THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. XLIV.—JOHN TORREY: *A Biographical Notice.*

THE following article forms a part of the Annual Report by the Council to the American Academy of Arts and Sciences, before which it was read at the meeting on the 8th of April, ult. This accounts for the form in which the biography is cast, and for the exclusion of many details and personal particulars which otherwise would naturally have found a place in it. It is the President of the American Academy rather than the companion and friend of many years who writes; yet the narrative must needs take tone and color from the intimate association of the writer with the subject of it. A. GRAY.

JOHN TORREY, M.D., LL.D., died at New York, on the 10th of March, 1873, in the 77th year of his age. He has long been the chief of American botanists, and was at his death the oldest, with the exception of the venerable ex-president of the American Academy (Dr. Bigelow), who entered the botanical field several years earlier, but left it to gather the highest honors and more lucrative rewards of the medical profession, about the time when Dr. Torrey determined to devote his life to scientific pursuits.

The latter was of an old New England stock, being, it is thought, a descendant of William Torrey, who emigrated from

Combe St. Nicholas, near Chard, in Somersetshire, and settled at Weymouth, Massachusetts, about the year 1640.*

His grandfather, John Torrey, with his son, William, removed from Boston to Montreal at the time of the enforcement of the "Boston Port bill." But neither of them was disposed to be a refugee. For the son, then a lad of 17 years, ran away from Canada to New York, joined his uncle, Joseph Torrey, a Major of one of the two light infantry regiments of regulars (called Congress's own) which were raised in that city; was made an ensign, and was in the rear-guard of his regiment on the retreat to White Plains; served in it throughout the war with honor, and until at the close he re-entered the city upon "Evacuation Day," when he retired with the rank of Captain. Moreover, the father soon followed the son and became quartermaster of the regiment. Captain Torrey, in 1791, married Margaret Nichols, of New York.

The subject of this biographical notice was the second of the issue of this marriage, and the oldest child who survived to manhood. He was born in New York, on the 15th of August, 1796. He received such education only as the public schools of his native city then afforded, and was also sent for a year to a school in Boston. When he was 15 or 16 years old his father was appointed Fiscal Agent of the State Prison at Greenwich, then a suburban village, to which the family removed.

* In some notes furnished by a member of the family, the descent is endeavored to be traced through the eldest of the five sons who survived their parent, namely, Samuel, who came with him from England, became a minister of the gospel, and had the unprecedented honor of preaching three election sermons (in 1674, 1683, and 1695), as well as of having three times declined the presidency of Harvard College (after Hoar, after Oakes, and after Rogers). Although educated at the College, he was not a graduate, because he left it in 1650, after three years residence, just when the term for the A.B. degree was lengthened to four years. The tradition has it, that, "at the prayer meetings of the students, he was generally invited to make the concluding prayer,"—for which an obvious reason suggests itself,—for "such was his devotion of spirit that, after praying for two hours, the regret was that he did not continue longer." Students of the present day are probably less exacting.

The desire to claim a descent through so eminent a member of the family is natural. But our late venerable associate, Mr. Savage, in his Dictionary of early New England families, states that he could not ascertain that Samuel had any children.

At this early age he chanced to attract the attention of Amos Eaton, who soon afterwards became a well-known pioneer of natural science, and with whom it may be said that popular instruction in natural history in this country began. He taught young Torrey the structure of flowers and the rudiments of botany, and thus awakened a taste and kindled a zeal which were extinguished only with his pupil's life. This fondness soon extended to mineralogy and chemistry, and probably determined the choice of a profession. In the year 1815, Torrey began the study of medicine in the office of the eminent Dr. Wright Post, and in the College of Physicians and Surgeons, in which the then famous Dr. Mitchill and Dr. Hosack were professors of scientific repute; he took his medical degree in 1818; opened an office in his native city, and engaged in the practice of medicine with moderate success, turning the while his abundant leisure to scientific pursuits, especially to botany. In 1817, while yet a medical student, he reported to the Lyceum of Natural History —of which he was one of the founders—his Catalogue of the Plants growing spontaneously within thirty miles of the city of New York, which was published two years later; and he was already, or very soon after, in correspondence with Kurt Sprengel and Sir James Edward Smith abroad, as well as with Elliot, Nuttall, Schweinitz, and other American botanists. Two mineralogical articles were contributed by him to the very first volume of the American Journal of Science and Arts (1818–1819), and several others appeared a few years later, in this and in other Journals.

Elliott's sketch of the Botany of South Carolina and Georgia was at this time in course of publication, and Dr. Torrey planned a counterpart systematic work upon the botany of the Northern States. The result of this was his "Flora of the Northern and Middle Sections of the United States, i. e., north of Virginia,"—which was issued in parts, and the first volume concluded in the summer of 1824. In this work Dr. Torrey first developed his remarkable aptitude for descriptive botany, and for the kind of investigation and discrimination, the tact and acumen, which it calls for. Only those few,—now, alas, very few,—surviving botanists who used this book through the following years can at all appreciate its value and

influence. It was the fruit of those few but precious years which, seasoned with pecuniary privation, are in this country not rarely vouchsafed to an investigator, in which to prove his quality before he is haply overwhelmed with professional or professorial labors and duties.

In 1824, the year in which the first volume (or nearly half) of his Flora was published, he married Miss Eliza Robinson Shaw, of New York, and was established at West Point, having been chosen Professor of Chemistry, Mineralogy and Geology in the United States Military Academy. Three years later he exchanged this chair for that of Chemistry and Botany (practically that of Chemistry only, for Botany had already been allowed to fall out of the medical curriculum in this country) in the College of Physicians and Surgeons, New York, then in Barclay Street. The Flora of the Northern States was never carried further; although a "Compendium," a pocket volume for the field, containing brief characters of the species which were to have been described in the second volume, along with an abridgement of the contents of the first, was issued in 1826. Moreover, long before Dr. Torrey could find time to go on with the work, he foresaw that the natural system was not much longer to remain, here and in England, an esoteric doctrine, confined to profound botanists, but was destined to come into general use and to change the character of botanical instruction. He was himself the first to apply it in this country in any considerable publication.

The opportunity for this, and for extending his investigations to the great plains and the Rocky Mountains on their western boundary, was furnished by the collections placed in Dr. Torrey's hands by Dr. Edwin James, the botanist of Major Long's expedition in 1820. This expedition skirted the Rocky Mountains belonging to what is now called Colorado Territory, where Dr. James, first and alone, reached the charming alpine vegetation, scaling one of the very highest summits, which from that time and for many years afterward was appropriately named James' Peak; although it is now called Pike's Peak, in honor of General Pike, who long before had probably seen, but had not reached it.

As early as the year 1823 Dr. Torrey communicated to the Lyceum of Natural History descriptions of some new species of James's collection, and in 1826 an extended account of all the plants collected, arranged under their natural orders. This is the earliest treatise of the sort in this country, arranged upon the natural system; and with it begins the history of the botany of the Rocky Mountains, if we except a few plants collected early in the century by Lewis and Clark, where they crossed them many degrees farther north, and which are recorded in Pursh's Flora. The next step in the direction he was aiming was made in the year 1831, when he superintended an American reprint of the first edition of Lindley's Introduction to the Natural System of Botany, and appended a catalogue of the North American genera arranged according to it.

Dr. Torrey took an early and prominent part in the investigation of the United States species of the vast genus *Carex*, which has ever since been a favorite study in this country. His friend, von Schweinitz, of Bethlehem, Penn., placed in his hands and desired him to edit, during the author's absence in Europe, his Monograph of North American Carices. It was published in the Annals of the New York Lyceum, in 1825, much extended, indeed almost wholly rewritten, and so much to Schweinitz's satisfaction that he insisted that this classical Monograph "should be considered and quoted in all respects as the joint production of Dr. Torrey and himself." Ten or eleven years later, in the succeeding volume of the Annals of the New York Lyceum, appeared Dr. Torrey's elaborate Monograph of the other North American Cyperaceæ, with an appended revision of the Carices, which meanwhile had been immensely increased by the collections of Richardson, Drummond, &c., in British and Arctic America. A full set of these was consigned to his hands for study (along with other important collections), by his friend Sir Wm. Hooker, upon the occasion of a visit which he made to Europe in 1833. But Dr. Torrey generously turned over the Carices to the late Professor Dewey, whose rival Caricography is scattered through forty or fifty volumes of the American Journal of Science and Arts; and so had only to sum up the results in this regard, and

add a few southern species at the close of his own Monograph of the order.

About this time, namely in the year 1836, upon the organization of a geological survey of the State of New York upon an extensive plan, Dr. Torrey was appointed Botanist, and was required to prepare a Flora of the State. A laborious undertaking it proved to be, involving a heavy sacrifice of time, and postponing the realization of long-cherished plans. But in 1843, after much discouragement, the Flora of the State of New York, the largest if by no means the most important of Dr. Torrey's works, was completed and published, in two large quarto volumes, with 161 plates. No other State of the Union has produced a Flora to compare with this. The only thing to be regretted is that it interrupted, at a critical period, the prosecution of a far more important work.

Early in his career Dr. Torrey had resolved to undertake a general flora of North America, or at least of the United States, arranged upon the natural system, and had asked Mr. Nuttall to join him, who, however, did not consent. At that time, when little was known of the regions west of the valley of the Mississippi, the ground to be covered and the materials at hand were of comparatively moderate compass; and in aid of the northern part of it, Sir William Hooker's Flora of British America—founded upon the rich collections of the Arctic explorers, of the Hudson's Bay Company's intelligent officers, and of such hardy and enterprising pioneers as Drummond and Douglas,—was already in progress. At the actual inception of the enterprise, the botany of Eastern Texas was opened by Drummond's collections, as well as that of the coast of California by those of Douglas, and afterward those of Nuttall. As they clearly belonged to our own phyto-geographical province, Texas and California were accordingly annexed botanically before they became so politically.

While the field of botanical operations was thus enlarging, the time which could be devoted to it was restricted. In addition to his chair in the Medical College, Dr. Torrey had felt obliged to accept a similar one at Princeton College, and to all was now added, as we have seen, the onerous post of State Botanist. It was in the year 1836 or 1837 that he invited the

writer of this notice—then pursuing botanical studies under his auspices and direction—to become his associate in the Flora of North America. In July and in October, 1838, the first two parts, making half of the first volume, were published. The great need of a full study of the sources and originals of the earlier-published species was now apparent; so, during the following year, his associate occupied himself with this work in the principal herbaria of Europe. The remaining half of the first volume appeared in June, 1840. The first part of the second volume followed in 1841; the second in the spring of 1842; and in February, 1843, came the third and the last; for Dr. Torrey's associate was now also immersed in professorial duties and in the consequent preparation of the works and collections which were necessary to their prosecution.

From that time to the present the scientific exploration of the vast interior of the continent has been actively carried on, and in consequence new plants have poured in year by year in such numbers as to overtask the powers of the few working botanists of the country, nearly all of them weighted with professional engagements. The most they could do has been to put collections into order in special reports, revise here and there a family or a genus monographically, and incorporate new materials into older parts of the fabric, or rough-hew them for portions of the edifice yet to be constructed. In all this Dr. Torrey took a prominent part down almost to the last days of his life. Passing by various detached and scattered articles upon curious new genera and the like, but not forgetting three admirable papers published in the Smithsonian Contributions to Knowledge (Plantæ Fremontianæ, and those on *Batis* and *Darlingtonia*), there is a long series of important, and some of them very extensive, contributions to the reports of government explorations of the western country,—from that of Long's expedition already referred to, in which he first developed his powers, through those of Nicollet, Fremont, and Emory, Sitgreaves, Stansbury, and Marcy, and those contained in the ampler volumes of the Surveys for Pacific Railroad routes, down to that of the Mexican Boundary, the botany of which forms a bulky quarto volume, of much interest. Even at the last, when he rallied transiently from the fatal attack, he took

in hand the manuscript of an elaborate report on the plants collected along our Pacific coast in Admiral Wilkes's celebrated expedition, which he had prepared fully a dozen years ago, and which (except as to the plates) remains still unpublished through no fault of his. There would have been more to add, perhaps of equal importance, if Dr. Torrey had been as ready to complete and publish, as he was to investigate, annotate and sketch. Through undue diffidence and a constant desire for a greater perfection than was at the time attainable, many interesting observations have from time to time been anticipated by other botanists.

All this botanical work, it may be observed, has reference to the Flora of North America, in which, it was hoped, the diverse and separate materials and component parts, which he and others had wrought upon, might some day be brought together in a completed system of American botany.

It remains to be seen whether his surviving associate of nearly forty years will be able to complete the edifice. To do this will be to supply the most pressing want of the science, and to raise the fittest monument to Dr. Torrey's memory.

In the estimate of Dr. Torrey's botanical work, it must not be forgotten that it was nearly all done in the intervals of a busy professional life; that he was for more than thirty years an active and distinguished teacher, mainly of chemistry, and in more than one institution at the same time; that he devoted much time and remarkable skill and judgment to the practical applications of chemistry, in which his counsels were constantly sought and too generously given; that when, in 1857, he exchanged a portion, and a few years later the whole, of his professional duties for the office of U. S. Assayer, these requisitions upon his time became more numerous and urgent.* In addition to the ordinary duties of his office, which he fulfilled to the end with punctilious faithfulness (signing the last of his

* It ought to be added, that, when the Government Assay Office at New York was established, the Secretary of the Treasury selected Dr. Torrey to be its Superintendent,—which would have given to the establishment the advantage of a scientific head. But Dr. Torrey resolutely declined the less laborious and better paid post, and took in preference one the emoluments of which were much below his worth and the valuable extraneous services he rendered to the Government,—simply because he was unwilling to accept the care and responsibility of treasure.

daily reports upon the very day of his death, and quietly telling his son and assistant that it would not be necessary to bring him any more), he was frequently requested by the head of the Treasury Department to undertake the solution of difficult problems, especially those relating to counterfeiting, or to take charge of some delicate or confidential commission, the utmost reliance being placed upon his skill, wisdom, and probity.

In two instances these commissions were made personally gratifying, not by pecuniary payment, which, beyond his simple expenses, he did not receive, but by the opportunity they afforded to recruit failing health and to gather floral treasures. Eight years ago he was sent by the Treasury Department to California by way of the Isthmus; and last summer he went again across the continent, and in both cases enjoyed the rare pleasure of viewing in their native soil, and plucking with his own hands, many a flower which he had himself named and described from dried specimens in the herbarium, and in which he felt a kind of paternal interest. Perhaps this interest culminated last summer, when he stood on the flank of the lofty and beautiful snow-clad peak to which a grateful former pupil and ardent explorer, ten years before, gave his name, and gathered charming alpine plants which he had himself named forty years before, when the botany of the Colorado Rocky Mountains was first opened. That age and fast-failing strength had not dimmed his enjoyment, may be inferred from his remark when, on his return from Florida the previous spring, with a grievous cough allayed, he was rallied for having gone to seek Ponce de Leon's fountain of Youth. "No," said he, "give me the fountain of Old Age. The longer I live, the more I enjoy life." He evidently did so. If never robust, he was rarely ill, and his last sickness brought little suffering and no diminution of his characteristic cheerfulness. To him, indeed, never came the "evil days" of which he could say, "I have no pleasure in them."

Evincing in age much of the ardor and all of the ingenuousness of youth, he enjoyed the society of young men and students, and was helpful to them long after he ceased to teach,—if, indeed, he ever did cease. For, as Emeritus Professor in Columbia College (with which his old Medical School was

united), he not only opened his herbarium, but gave some lectures almost every year, and as a trustee of the college for many years he rendered faithful and important service. His large and truly invaluable herbarium, along with a choice botanical library, he several years ago made over to Columbia College, which charges itself with its safe preservation and maintenance.

Dr. Torrey leaves three daughters, a son, who has been appointed U. S. Assayer in his father's place, and a grandson.

This sketch of Dr. Torrey's public life and works, which it is our main duty to exhibit, would fall short of its object if it did not convey, however briefly and incidentally, some just idea of what manner of man he was. That he was earnest, indefatigable, and able, it is needless to say. His gifts as a teacher were largely proved and are widely known through a long generation of pupils. As an investigator, he was characterized by a scrupulous accuracy, a remarkable fertility of mind, especially as shown in devising ways and means of research, and perhaps by some excess of caution.

Other biographers will doubtless dwell upon the more personal aspects and characteristics of our distinguished and lamented associate. To them, indeed, may fittingly be left the full delineation and illustration of the traits of a singularly transparent, genial, delicate and conscientious, unselfish character, which beautified and fructified a most industrious and useful life, and won the affection of all who knew him. For one thing, they cannot fail to notice his thorough love of truth for its own sake, and his entire confidence that the legitimate results of scientific inquiry would never be inimical to the Christian religion, which he held with an untroubled faith, and illustrated, most naturally and unpretendingly, in all his life and conversation. In this, as well as in the simplicity of his character, he much resembled Faraday.

Dr. Torrey was an honorary or corresponding member of a goodly number of the scientific societies of Europe, and was naturally connected with all prominent institutions of the kind in this country. He was chosen into the American Academy in the year 1841. He was one of the corporate members of the National Academy at Washington. He presided in his turn

over the American Association for the Advancement of Science; and he was twice, for considerable periods, President of the New York Lyceum of Natural History, which was in those days one of the foremost of our scientific societies. It has been said of him that the sole distinction on which he prided himself was his membership in the order of the Cincinnati, the only honor in this country which comes by inheritance.

As to the customary testimonial which the botanist receives from his fellows, it is fortunate that the first attempts were nugatory. Almost in his youth a genus was dedicated to him by his correspondent, Sprengel: this proved to be a *Clerodendron*, misunderstood. A second, proposed by Rafinesque, was founded on an artificial dismemberment of *Cyperus*. The ground was clear, therefore, when, thirty or forty years ago, a new and remarkable evergreen tree was discovered in our own Southern States, which it was at once determined should bear Dr. Torrey's name. More recently a congener was found in the noble forests of California. Another species had already been recognized in Japan, and lately a fourth in the mountains of Northern China. All four of them have been introduced and are greatly prized as ornamental trees in Europe. So that, all round the world, *Torreya taxifolia*, *Torreya Californica*, *Torreya nucifera*, and *Torreya grandis*—as well as his own important contributions to botany, of which they are a memorial—should keep our associate's memory as green as their own perpetual verdure.

ART. XLV.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXVI.—*On a compact Anglesite from Arizona;* by GEO. J. BRUSH.

ASSOCIATED with some specimens of galena from Castle Dome District, Arizona, there is found a compact banded mineral very much resembling some of the varieties of wood-tin from Cornwall. Mr. John C. Trautwine, C.E., of Philadelphia, having observed this anomalous substance, sent some specimens of it to me for determination in October last. A pyrognostic examination made at that time showed the mineral to be a compact anglesite. Subsequently Mr. Trautwine kindly provided me with more specimens, and a quantitative examination

by Mr. Samuel T. Tyson, of this laboratory, has confirmed the correctness of the first examination.

The compact anglesite occurs in banded layers, sometimes with a nucleus of unoxidized and perfectly bright cleavable galena; while in other specimens the galena has entirely disappeared and the bands are symmetrically arranged in continuous circular or elliptical lines, as so often seen in agate. The bands or layers next the galena are frequently almost black, fading from a dark brownish-gray to a light grayish-white at the point farthest from the nucleus of galena, and the outer layer is sometimes exteriorly coated with minute, almost microscopic, crystals of transparent colorless anglesite. The thickness of these layers of compact anglesite in the specimens examined was from one-half to one inch. The specific gravity of the light colored variety was about 6, while some of the dark mineral gave a density as high as 6·44. Hardness = 3.

Mr. Tyson digested the finely pulverized mineral with a strong solution of bicarbonate of soda for 24 hours, then filtered and washed thoroughly and determined the sulphuric acid by precipitation as a baryta salt. The residue produced by the action of the bicarbonate was boiled with acetic acid, and the insoluble portion was collected on a weighed filter. The lead was then thrown down from the acetic solution by sulphuric acid. Four analyses made by this method gave Mr. Tyson.

	Dark variety.		Light variety.	
	1	2	1	2
Oxide of lead,	72·53	72·62	72·34	72.53
Sulphuric acid,	26·43	26·33	26·29	26·28
Insoluble residue,	0·75	0·73	0.83	0·75
	99·71	99·68	99·46	99·56

A fire-assay of the light variety yielded Mr. Tyson 0·0578 per cent silver, 16·87 ounces per ton of 2,000 lbs., while the galena was found to contain 27·3 ounces per ton, thus proving that a considerable portion of the silver was lost in the process of oxidation. The insoluble residue in the dark variety was almost black, and a qualitative examination of it showed it to be chiefly sulphide of lead, while the residue from the light-colored mineral was nearly white and almost entirely insoluble in acids, and proved to consist mainly of clay.

Sheffield Laboratory, New Haven, April, 1873.

ART. XLVI.—*On some Results of the Earth's Contraction from cooling, including a discussion of the Origin of Mountains, and the nature of the Earth's Interior;* by JAMES D. DANA.

PART I.

PREPARATORY to a discussion of some questions connected with the earth's contraction, I here present a statement of the views which I have entertained with regard to the prominent results of this agency. They first appeared in 1846 and 1847, in volumes ii, iii and iv of the second series of this Journal, and were somewhat extended in 1856, in vol. xxii.* Full credit is given to earlier writers in connection with the articles referred to. The views are as follows:†—

1. The defining of the continental and oceanic areas began with the commencement of the earth's solidification at surface, as proved by the system of progress afterward.

2. The continental areas are the areas of least contraction, and the oceanic basins those of greatest, the former having earliest had a solid crust. After the continental part was thus stiffened, and rendered comparatively unyielding, the oceanic part went on cooling, solidifying, and contracting throughout; consequently it became depressed, with the sides of the depression somewhat abrupt. The formation of the oceanic basins and continental areas was thus due to "unequal radial contraction."‡

* Volume ii, 385; iii, 94, 176, 380: iv, 88; xxii, 305, 335.

† I may add in this place that a sight of Mädler's chart of the Moon in 1846, six years after my visit to the crater of Kilauea, in the Wilkes Exploring Expedition, prompted to the first of the articles on the subject—that on the Volcanoes of the Moon (II. ii, 335, 1846)—in which the origin of continents and oceanic basins is considered. The most important of Prevost's papers, on the origin of mountains had been published six years before, but I knew nothing of his views until after my paper was ready for publication, as I remark in a paragraph near its close.

‡ The principle thus expressed by Prof. LeConte in volume iv of this Journal, (1872,) does not differ essentially from my old view, except that it is connected with the idea of a solid globe. Prof. LeConte, on p. 466 of his article, attributes to me the opinion that the "sinking of sea bottoms, determined by interior contraction, is the [source of the] force by which continents are elevated." But I have never referred the origin of continents to such a cause, or to any other than that stated above.

Moreover, the elevation of mountains on the borders of continents I have attributed, not to "sinking sea-bottoms" merely, but to lateral pressure produced by contraction over continental as well as oceanic areas, that on the oceanic being made much the greatest, as stated beyond. My language is frequently ambiguous on this last point, because I speak of the oceanic as the "subsiding" areas. But the term is used relatively. In volume iii, on p. 179, (1847,) I observe that mountain elevations, occur "near the limit between the great contracting and the non-contracting (comparatively non-contracting) areas:" and in various places I describe the contraction as general. In my Manual of Geology, on page 732, I remark that the elevating "force acted most strongly from the oceanic direction," which was the idea throughout. I do not deny, however, that I have supposed too

3. The principal mountain chains are portions of the earth's crust which have been pushed up, and often crumpled or plicated, by the lateral pressure resulting from the earth's contraction.

4. (*a*) Owing to the lateral pressure* from contraction over both the continental and oceanic areas, and to the fact that the latter are the regions of greatest contraction and subsidence, and that their sides pushed, like the ends of an arch, against the borders of the continents, therefore, along these borders, within 300 to 1000 miles of the coast, a continent experienced its profoundest oscillations of level, had accumulated its thickest deposits of rocks, underwent the most numerous uplifts, fractures and plications, had raised its highest and longest mountain chains, and became the scene of the most extensive metamorphic operations, and the most abundant outflows of liquid rock.

And (*b*) since the most numerous and closest plications, the great est ranges of volcanoes, the largest regions of igneous eruption and metamorphic action, exist on the *oceanic slope* of the border mountain chains, instead of the continental, therefore the lateral pressure acted most effectively in a direction *from* the ocean.

(*c*) Since these border features are vastly grander along that border of a continent which faces the largest ocean, therefore, the lateral pressure against the sides of a continent was most effective on the border of the largest oceanic basin, and for the two, the Pacific and Atlantic, was approximately proportioned to the extent of the basins; this being due to the fact that the oceanic were the subsiding areas, that is, those which contracted most, and that the larger area became the most depressed.

5. The oscillations of level that have taken place over the interior of North America, through the geological ages, have in some degree conformed in direction of axis to those of the border regions, all being parts fundamentally of two systems of movements, one dominantly in a direction northwestward or from the Atlantic, the other northeastward or from the Pacific.

6. Owing to the approximate uniformity of direction in the lateral thrust under these two systems through the successive ages, (a consequence of the isolated position of the continent between two oceanic basins, transverse to one another in axial direc-

large a part of the lateral force to have come from the special contraction and consequent subsidence of the oceanic part of the globe.

Professor N. S. Shaler in 1866 (Proc. Boston N. H. Soc., x, 237, xi, 8, and Geol. Mag., v, 511) presented, *as original*, the idea that "mountain chains are only folds of the outer portion of the crust caused by the contraction of the lower regions of the outer shell;" and that "the subsidence of ocean floors would, by producing fractions and dislocations along shore lines, tend to originate mountain chains along sea-borders and approximately parallel to them;" which is essentially the view that LeConte attributes to me. These ideas are coupled with others respecting limitations of the action of contraction due to denudation and deposition, in which I have no share.

* In my papers in 1847 I used the terms lateral pressure, lateral force, tension, horizontal force, force acting tangentially, as synonyms. "Lateral pressure" was the term oftenest employed, and it was explained by reference to a Prince Rupert's drop. (See this Journ., II, iii, 96, etc.) The action appealed to was not in any way different from the "tangential thrust" of Mallet.

tion,) mountains *of different ages* on the same border, or part of a border, have approximately the *same trend*, and those *of the same age* on the opposite border—Pacific and Atlantic—have in general *a different and nearly transverse trend.* Hence, "one dial plate for the mountains of the world, such as Elie de Beaumont deduced mainly from European geology, will not mark time for America." (This Journ., II, iii, 398, 1847; xxii, 346, 1856.)

7. The features of the North American continent were to a great extent defined in pre-Silurian time, the course of the Azoic, from the Great Lakes to Labrador, being that of the Appalachians, and various ridges in the Rocky Mountains foreshadowings of this great chain, and so on in many lines over the continental surface; and thus its adult characteristics were as plainly manifested in its beginnings as are those of a vertebrate in a half-developed embryo.

8. Metamorphism of regions of strata has taken place only during periods of disturbance, or when plication and faults were in progress; all metamorphic regions being regions of disturbed and generally of plicated rocks.

The heat required for alteration came up from the earth's liquid interior. (This part of the view requires modification, while the other part, I believe, remains good.)

9. The volcanoes of the continental areas are mostly confined to the sea-borders, or the oceanic slope of the border mountain chains, not because of the vicinity of salt water, but because these were the regions of greatest disturbance and fractures through lateral pressure. Volcanoes are indexes of danger, never "safety-valves."

10. Earthquakes were a result of sudden fracturings and dislocations proceeding from lateral pressure. In vol. iii, p. 181, (1847,) occurs the remark: "We see that the lateral pressure exerted would be likely to dislocate," and in the next line, "such fissurings, whether internal or external, would cause shakings of the earth (*earthquakes*) of great violence, and in all periods of the earth's history, and it might be over a hemisphere at once."

Another important subject—that of the systems in the trends of feature lines over the globe—is discussed in the articles referred to; but I pass it by for the present.

I propose to bring the above principles under consideration with reference to making such changes as may now be necessary.

I take up, first, the question as to whether oscillations of level, that is, subsidences and elevations, have been made by the lateral pressure resulting from contraction, as is assumed in my writings on the subject and those of most other authors;—and how was the lateral thrust from the direction of the oceanic areas made to differ in its results from that from the opposite direction? After which I shall pass to the subjects of metamorphism, igneous eruptions, volcanoes, the earth's interior, and the origin of oceanic basins.

1. *Have subsidences been produced by lateral pressure?*

The theory of Professor James Hall, that the great subsidences of the globe have been made by the gravity of accumulating sediments, has been shown elsewhere* to be wholly at variance with physical law.

Another theory is presented by Prof. LeConte, in his recent paper in the last volume of this Journal, to which the reader is referred. Admitting, with Prof. Hall, that the mean thickness of the accumulations in the Appalachian region of Pennsylvania is 40,000 feet, and therefore that this is the measure of the gradual subsidence that attended their deposition, he shows that the temperature in the bottom deposits would have been, supposing the usual rate of increase downward (1° F. for 58 feet of descent), 800° F., and, at 10,000 feet, 230° F.; and he argues that hence there would have resulted below, first, "lithification and therefore increasing density, and therefore *contraction and subsidence pari passu* with the deposit;" next, or at a greater depth, "aqueo-igneous softening" or "melting," the temperature of 800° F. being "certainly sufficient to produce this result as well as metamorphism, and, during this process, the subsidence would probably continue;" and, in addition, the underlying strata on which the sediments were deposited would have participated in the aqueo-igneous fusion" and thus have added to the result.†

No other cause of the gradual subsidence than that here cited is appealed to.

Now *the whole of this contraction took place*, if any occurred, *in the underlying Archæan rocks* (Azoic, or Laurentian and Huro-

* This Journal, II, xlii, 210, 1866, III, v, 347, 1873; LeConte, ib., III, iv, 461, 1872.

† The principal points in Prof. Hall's theory of mountains, published in 1859, (see p. 347, of this volume,) are:

1. Coast regions the courses of marine currents, and hence of deposited sediments.

2. The accumulation of sediments by their gravity gradually sink the crust, and thus a great thickness is attained; the rocks become solidified and sometimes crystallized below.

3. The continents afterward somehow raised—not the mountain regions separately.

4. Shaping of the mountains out of other sediments by denudation.

5. Metamorphism due to "motion," "fermentation," and a little heat; the heat coming up from below (the isogeothermal planes rising) in consequence of the increasing accumulations at surface.

In Prof. LeConte's theory (this Journ., III, iv, 345, 460, 1872):

1. The same as in Prof. Hall's.

2. As explained in the text above.

3. After an aqueo-igneous softening of the beds below, the lateral thrust from the earth's contraction pressed together the region of sedimentary accumulation, plicating and crushing the beds.

4. The elevation of mountains due solely to crushing and plication.

5. Metamorphism consequent on the heating derived by the rise of the isogeothermal planes.

nian); for in obtaining by measurement this thickness, 40,000 feet, the *contracted* rocks were measured.

The 40,000 feet of subsidence required was therefore wholly independent of contraction in the stratified sediments. But these underlying Archæan rocks were probably crystallized before the Paleozoic era began; for in New York and New Jersey they are in this condition, and they underlie the Silurian rocks unconformably; and the New Jersey Archæan or Highland region is but a northern part of that of Pennsylvania and Virginia. They would consequently have expanded with the heat instead of contracting. Even if not crystallized, they would have been well compacted under the enormous weight of 40,000 feet of strata, and no experiments on rocks that I have met with authorize the assumption that the ordinary law of expansion from heat would have been set aside.

For further argument on this point I refer to the subsidence in the Connecticut valley during the era of the Connecticut River sandstone (supposed to be Triassico-Jurassic). The thickness of rock produced in the era was probably about 4,000 feet, and this is the extent therefore of the registered subsidence. The sandstone strata, as is apparent in many places, rests on the upturned metamorphic rocks—gneiss, mica schist, etc.,—of Paleozoic or earlier age. As shown in the preceding paragraph, the contraction, under Prof. LeConte's principle, must have been confined to the underlying rocks; and since these are crystalline metamorphic schists, and the depth of sandstone was not sufficient to raise much the temperature within them (the rocks are in general little compacted and often feebly solidified), the heat ascending from below as accumulation went on above would have produced expansion instead of contraction.

Without further reference to facts, it is, I think, clear that the subsidence required could not be obtained by the method appealed to by Prof. LeConte. Whatever cause, in either of the above cases, occasioned the subsidence, it must have been one that could do its work in spite of opposition on the part of the heat in the rocks themselves or those below.

Another cause of local subsidence is local cooling beneath, accompanying the increasing accumulation of sediments. But this idea is too obviously absurd to require remark.

In the present state of science, then, no adequate cause of subsidence has been suggested apart from the old one of lateral pressure in the contracting material of the globe.

2. *Have elevations been produced directly by lateral pressure?*

The theory of Prof. Hall denies that mountains are a result of *local* elevations, or of any elevation apart from a general continental. This hypothesis I have elsewhere discussed.*

* This Journal, II, xlii, 205, 252, and this volume, p. 347.

Prof. LeConte makes the elevation of mountains real, but, after explaining that the crushing effects of lateral thrust would necessarily cause a lengthening upward of the compressed strata (as in the compression of slate rocks attending the production of slaty cleavage), and thereby produce a large amount of actual elevation, arrives at the view, that there is no permanent elevation beyond what results from crushing. With crushing, in this action, plication is associated; but it should have a larger place than his words seem to give it (in all plication the rocks over a region being pressed into a narrower space, which could be done only by adding to the height), as it has performed ten-fold more work of this kind than crushing.

But are plication and crushing the only methods of producing, under lateral pressure, the actual elevations of mountain regions? Is there not real elevation besides?

In the later part of the Post-tertiary or Quaternary era, the region about Montreal was raised nearly 500 feet, as shown by the existence of sea-beaches at that height; and similiar evidence proves that the region about Lake Champlain was raised at the same time at least 300 feet, and the coast of Maine 150 to 200 feet. Hence the region raised was large. No crushing or plication of the upper rocks occurred, and none in the under rocks could well have taken place without exhibitions at surface; and this cause, therefore, cannot account for the elevation. The elevated sea-border deposits of the region are in general horizontal. This example is to the point as much as if a mountain had been made by the elevation.

But we have another example on a mountain scale, and one of many. Fossiliferous beds over the higher regions of the Rocky mountains are unquestioned evidence that a large part of this chain has been raised 8,000 to 10,000 feet above the ocean level since the Cretaceous era.* The Cretaceous rocks, to which these fossiliferous beds belong, were upturned in the course of the slowly progressing elevation, and so also were part of the Tertiary beds—for the elevation went forward through the larger part, or all, of the Tertiary era. But the local crushing or plication of these beds cannot account for the elevation, and no other crushing among the surface rocks of the mountains can be referred to this era. There may have been a crushing and crumpling of the nether rocks of the mountain. But it must also be admitted that there might have been, under tangential pressure, a bending of the strata without crushing,

* The height of the Cretaceous (stratum No. 2 of the Upper Missouri Cretaceous) at Aspen, in Wyoming, is full 8,000 feet above tide level (Meek). Beds occur also in South Park, Colorado, the height of which is 8500 feet; and, according to Hayden, in the region of the Wind River Mountains, the beds have a height of 10,000 to 11,000 feet above the sea.

especially if there is beneath the earth's rind along the continental borders a region or layer of "aqueo-igneous fusion," such as Prof. LeConte recognizes.

In the course of the geological history of the North American continent, there were many oscillations of level in the land. Portions that were raised above the sea-level in one era in another subsided again and sunk beneath it; and Prof. LeConte, in the course of his discussion, admits the existence of an elevated region along the Atlantic border which afterward disappeared. Had the elevation in the case of such oscillations been dependent on plication and crushing beneath, so complete a disappearance afterward would have been very improbable.

Such facts as the above appear to prove that elevatory movements have often been, like those of subsidence, among the direct results of lateral pressure. The facts are so well known and the demonstration so generally accepted as complete, that I have suspected that there is here an unintentional omission or oversight in Prof. LeConte's paper.

3. *Kinds and Structure of Mountains.*

While mountains and mountain chains all over the world, and low lands, also, have undergone uplifts, in the course of their long history, that are not explained on the idea that all mountain elevating is simply what may come from plication or crushing, the *component parts* of mountain chains, or those simple mountains or mountain ranges that are *the product of one process of making*—may have received, *at the time of their original making*, no elevation beyond that resulting from plication.

This leads us to a grand distinction in orography, hitherto neglected, which is fundamental and of the highest interest in dynamical geology; a distinction between—

1. A simple or *individual* mountain mass or range, which is the result of *one process of making*, like an individual in any process of evolution, and which may be distinguished as a *monogenetic* range, being *one in genesis;* and

2. A composite or *polygenetic* range or chain, made up of two or more monogenetic ranges combined.

The Appalachian chain—the mountain region along the Atlantic border of North America—is a *polygenetic* chain; it consists, like the Rocky and other mountain chains, of several *monogenetic* ranges, the more important of which are: 1. The Highland range (including the Blue Ridge or parts of it, and the Adirondacks also, if these belong to the same process of making) pre-Silurian in formation; 2. The Green Mountain range, in western New England and eastern New York, completed essentially after the Lower Silurian era or during its closing period; 3. The Alleghany range, extending from south-

ern New York southwestward to Alabama, and completed immediately after the Carboniferous age.

The making of the Alleghany range was carried forward at first through a long-continued subsidence—a *geosynclinal** (not a *true* synclinal, since the rocks of the bending crust may have had in them many true or simple synclinals as well as anticlinals), and a consequent accumulation of sediments, which occupied the whole of Paleozoic time; and it was completed, finally, in great breakings, faultings and foldings or plications of the strata, along with other results of disturbance. The folds are in several parallel lines, and rise in succession along the chain, one and another dying out after a course each of 10 to 150 miles; and some of them, if the position of the parts which remain after long denudation be taken as evidence, must have had, it has been stated, an altitude of many thousand feet; and there were also faultings of 8,000 to 10,000 feet, or, according to Lesley, of 20,000 feet.† This is one example of a *monogenetic* range.

The Green Mountains are another example in which the history was of the same kind: first, a slow subsidence or geosynclinal, carried forward in this case during the Lower Silurian era or the larger part of it; and, accompanying it, the deposition of sediments to a thickness equal to the depth of the subsidence; finally, as a result of the subsidence and as the climax in the effects of the pressure producing it, an epoch of plication, crushing, etc. between the sides of the trough.

In the Alleghany range the effects of heat were mostly confined to solidification; the reddening of such sandstones and shaly sandstones as contained a little iron in some form;‡ the coking of the mineral coal; and probably, on the western outskirts where the movements were small, the distillation of mineral oil, through the heating of shales or limestones containing carbohydrogen material, and its condensation in cavities among overlying strata; with also some metamorphism to the eastward; while in the making of the Green Mountains, there was metamorphism over the eastern, middle, and southern portions, and imperfect metamorphism over most of the western side to almost none in some western parts.

Another example is offered by the Triassico-Jurassic region of the Connecticut valley. The process included the same stages in kind as in the preceding cases. It began in a geosyn-

* From the Greek *γῆ*, *earth*, and *synclinal*, it being a bend in the earth's crust.

† See an admirable paper on these mountains by Professors W. B. and H. D. Rogers, in the Trans. Assoc. Amer. Geol. and Nat., 1840–42. J. P. Lesley gives other facts in his "Manual of Coal and its Topography," and in many memoirs in the Proceedings of the American Philosophical Society. A brief account is contained in the author's Manual of Geology.

‡ Oxide of iron produced by a wet process at a temperature even as low as 212° F. is the red oxide Fe_2O_3, or at least has a red powder. (Am. Jour. Sci., II, xliv, 292.)

clinal of probably 4,000 feet, this much being registered by the thickness of the deposits; but it *stopped short of metamorphism*, the sandstones being only reddened and partially solidified; and *short of plication or crushing*, the strata being only tilted in a monoclinal manner 15° to 25°; it ended in numerous great longitudinal fractures, as a final catastrophe from the subsidence, out of which issued the trap (dolerite) that now makes Mt. Holyoke, Mt. Tom, and many other ridges along a range of 100 miles.*

These examples exhibit the characteristics of a large class of mountain masses or ranges. A geosynclinal accompanied by sedimentary depositions, and ending in a catastrophe of plications and solidification, are the essential steps, while metamorphism and igneous ejections are incidental results. The process is one that produces final stability in the mass and its annexation generally to the more stable part of the continent, though not stable against future oscillations of level *of wider range*, nor against denudation.

It is apparent that in such a process of formation elevation by direct uplift of the underlying crust has no necessary place. The attending plications may make elevations on a vast scale and so also may the shoves upward along the lines of fracture, and crushing may sometimes add to the effect; but elevation from an upward movement of the downward bent crust is only an incidental concomitant, if it occur at all.

We perceive thus where the truth lies in Professor LeConte's important principle. It should have in view alone *monogenetic* mountains and these only *at the time of their making.* It will then read, plication and shovings along fractures being made more prominent than crushing:

Plication, shoving along fractures and crushing are the true sources of the elevation that takes place *during the making* of geosynclinal monogenetic mountains.

And the statement of Professor Hall may be made right if we recognize the same distinction, and, also, reverse the order and causal relation of the two events, accumulation and subsidence; and so make it read:

Regions of monogenetic mountains were, previous, and preparatory, to the making of the mountains, areas each of a slowly progressing geosynclinal, and, *consequently*, of thick accumulations of sediments.

The prominence and importance in orography of the mountain individualities described above as originating through a

* This history is precisely that which I have given in my Manual of Geology, though without recognizing the parallelism in stages with the history of the Alleghanies.

geosynclinal make it desirable that they should have a distinctive name; and I therefore propose to call a mountain range of this kind a *synclinorium*, from *synclinal* and the Greek ὄρος, *mountain*.

This brings us to another important distinction in orographic geology — that of a second kind of monogenetic mountain. The *synclinoria* were *made through a progressing geosynclinal*. Those of the second kind, here referred to, were *produced by a progressing geanticlinal*. They are simply the upward bendings in the oscillations of the earth's crust—the geanticlinal waves, and hardly require a special name. Yet, if one is desired, the term *anticlinorium*, the correlate of *synclinorium*, would be appropriate. Many of them have disappeared in the course of the oscillations; and yet, some may have been for a time—perhaps millions of years—respectable mountains. The "Cincinnati uplift," extending southwestward from southern Ohio (about Cincinnati) into Tennessee, and referred by Newberry and others to the close of the Lower Silurian, was made at the same time, or nearly, with the Green Mountains; but, while the latter range is a synclinorium, the former is a geanticlinal or an anticlinorium, and it is one of the few (probably few) permanent monogenetic elevations of this kind over the earth's surface. There may possibly have been crumpling or crushing in the deep-seated rocks below which determined its permanence. As far as the Paleozoic rocks constituting it go, it is a *simple* synclinal; but it is really a synclinal of the earth's crust, and hence wholly distinct from ordinary synclinals, or those subordinate among the plications in a synclinorium, like the synclinals of the Alleghanies.

The geosynclinal ranges or synclinoria have experienced in almost all cases, since their completion, true elevation through great geanticlinal movements, but movements that embraced a wider range of crust than that concerned in the preceding geosynclinal movements, indeed a range of crust that comes strictly under the designation of a polygenetic mass. Thus the Connecticut valley sandstone beds, which must have been but little raised by the slight upturning they underwent at the epoch of their disturbance (since there was then neither plication nor crushing) are now seven hundred feet higher above the sea-level in Massachusetts than near New Haven, Conn.; and this is owing, not to denudation but to a subsequent elevation in which much of New England participated—a true geanticlinal uplift. So it has been the world over. The great uplift of the Rocky Mountain region of more than 8,000 feet, which began after the Cretaceous, had nothing to do, as I have said, with crushing or plication, although there was disturbance of the beds in certain local Cretaceous and Tertiary areas; it

appears to have been a true geanticlinal elevation of the Rocky Mountain mass, itself mainly, if not wholly, a combination of synclinoria.

Geosynclinals and geanticlinals of low angle, like those of the present day, graduate insensibly into horizontal surfaces. The later oscillations in the world's history have taken in a vastly wider range of crust than those of early time. We cannot point to any geosynclinal in progress that is probably on the way to become the site of a new synclinorium. This comes from the fact already stated, that the completion of a synclinorium has generally consisted in the solidification as well as plication of the rocks, and the addition of the whole mountain region to the more stable portion of the earth's crust; and the further fact that this process has been often repeated in past time, until the crust has been so stiffened above, as well as below, that only feeble flexures of vast span are possible, even if the lateral pressure from contraction had not also declined in force.

4. *How was the lateral thrust from the direction of the ocean made to differ in its action or results from that from the opposite direction?*

The fact of a difference in the effects of the lateral thrust from the opposite directions, the oceanic and continental, is beyond question. The evidence may here be repeated.

The greatest of elevations as well as subsidences, and also of plications and igneous eruptions, have taken place on the continental borders or in their vicinity; they thus show that there is something peculiar along such regions. Again, the border mountains in North America are parallel to the axes of the adjoining oceans; and thereby at right angles, instead of parallel to one another. Again, the folds in the Appalachians are not symmetrical folds, but, instead, have one slope much steeper than the other, proving inequality in the action of lateral pressure from the continental and oceanic directions. Further, the larger ranges of uplifts and effects of heat occur on the *oceanic* slope of the principal border-mountain chain, instead of the continental slope, favoring the view that this lateral thrust was more effective in the direction from the ocean against the continents than in the opposite. Finally, there is the fact that the disturbances or effects of lateral thrust have been *very much the greatest* on the border of the *largest* oceans.

But has this greater effectiveness of lateral thrust from the direction of the ocean been due to a proportionally greater contraction and subsidence of the oceanic crust than the continental —the sinking causing the oceanic arch to press against the sides of the basin. I formerly made this the chief means of mountain lifting; and now, while not giving it so great prominence,

I believe it to be a true cause. It is certain that the depressing of the ocean's bed, like the raising of the continental areas, has been in progress through the ages. The great principal rise of the continent and continental mountains took place after the Cretaceous period or during the Tertiary, and some of it even in the Quaternary; and this is almost positive demonstration that the bottoms of the oceans were tending downward cotemporaneously. It is not possible in the nature of contraction that it should have been all accomplished in these basins at the beginning of their existence—a point I shall further illustrate when discussing the nature of the earth's interior. Moreover, the mobile waters that occupy the oceanic depressions would have given important aid in the cooling of the underlying crust. It is to be noted, also, that the distance between the axis of the Appalachians in North America, and the opposite (African) side of the Atlantic is 4000 miles; and that between the axis of the Rocky Mountains and the opposite (Australian) coast of the Pacific is over 7000 miles, while between the axis of the Appalachians in Virginia and that of the Rocky Mountains in the same latitude, the distance is hardly 1500 miles. Hence the contraction was absolutely greatest over the oceanic areas, independently of any result from special causes; and if the generated pressure were not expended in uplifts over the oceanic areas themselves, it would have been in uplifts on its borders.

In addition to the above advantage which the oceanic areas have had in the making of border oscillations, the lower position of the oceanic crust, and the abruptness with which the sides fall off, give it an opportunity to push beneath the sides of the continents, and this would determine the production of such mountains and just such other effects of pressure, on the continental borders, as actually exist, even if contraction were equable over the globe, that is, were alike in rate over the oceanic and continental areas. It puts the oscillations over the continents inevitably under the direction of the adjoining oceanic crust. The angle of slope of the deepwater sides of the oceanic basin is generally above five degrees.*

* The angle of slope on the sides of the Oceanic basin has not yet been properly investigated. The margin of the basin on the Atlantic border is now in about 100 fathoms water (600 feet). According to soundings by the Coast Survey, as I am informed by Mr. A. Lidenkohl of the Coast Survey Office, through J. E. Hilgard, Esq., Assistant-in-Charge, the slope between 100 and 200 fathoms off Cape Hatteras is 2° 31′; off New York entrance, 2° 02′; off George's Shoal, 1° 35′. But for the region beyond 200 fathoms, the data are not sufficient for any certain conclusion. Mr. Lidenkohl observes: "If the soundings by Lieut. Murray off Cape Lookout can be trusted, the slope between the 100 and 2000 fathom line must be over 7 degrees. Berryman's soundings off St. George's Bank indicate a slope of about 3½ degrees. From this it may be inferred that the slope rather increases than decreases beyond the 200 fathom line."

This conclusion is further sustained by the known universality of oscillations over the oceanic basin. The central Pacific area of coral islands—"registers of subsidence"—stretches from the eastern Paumotus to the western Carolines, ninety degrees in longitude; and it indicates that the comparatively recent coral-island subsidence involved a region stretching over more than one-fourth the circumference of the globe. The fact teaches that the movements of the globe, which have been in progress through all time in obedience to the irresistible energy generated by contraction, have been world-wide, and so world-developing, even down to the latest era of geological history.

The above considerations sustain me in the opinion expressed in 1856 (this Journal, xxii, 335), that the relation in size between the mountains and the bordering oceans is not merely "formal," as pronounced by my friend Prof. LeConte, but has a *dynamical* significance.

In view of the considerations here presented, I believe there is no occasion to reject the fourth proposition (4 *a*) on page 424; but only to modify it as follows:

4*a*. Owing to the general contraction of the globe, the greater size of the oceanic than the continental areas, and the greater subsidence from continued contraction over the former than over the latter, and also to the fact that the oceanic crust had the advantage of *leverage*, or, more strictly, of obliquely upward thrust against the borders of the continents, because of its lower position, *therefore*, these borders within 300 to 1,000 miles of the coast, etc.

5. *Mountain-making slow work.*

To obtain an adequate idea of the way in which lateral pressure has worked, it is necessary to remember that mountain elevaion has taken place after immensely long periods of quiet and gentle oscillations. After the beginning of the Primordial, the first period of disturbance in North America of special note was that at the close of the Lower Silurian, in which the Green Mountains were finished; and if time from the beginning of the Silurian to the present included only fifty millions of years—which most geologists of the present day would consider much too small an estimate—the interval between the beginning of the Primordial and the uplifts and metamorphism of the Green Mountains, was at least ten millions of years. The next epoch of great disturbance in the same Appalachian region was that at the close of the Carboniferous era, in which the Alleghanies were folded up: by the above estimate of the length of time, thirty-five millions of years* after the commencement of the Silurian; so that the Appalachians were at least 35,000,000 of years in making, the preparatory subsidence having begun

* These estimates of the relative lengths of ages are based on the maximum thickness of their rocks—very uncertain data, but the best we have.

as early as the beginning of the Silurian. The next on the Atlantic border was that of the displacements of the Connecticut River Sandstone, and the accompanying igneous ejections, which occurred before the Cretaceous era:—at least seven millions of years, on the above estimate of the length of time, after the Appalachian revolution. Thus the lateral pressure resulting from the earth's contraction required an exceedingly long era in order to accumulate force sufficiently to produce a general yielding and plication or displacement of the beds, and start off a new range of prominent elevations over the earth's crust.

6. ***System in the mountain-making movements on the opposite borders of the North American Continent, and over the Oceanic areas.***

A summary of the general system of movements and mountain-making on the opposite borders of the continent, and over the oceanic areas, will, I think, render it apparent that the views here sustained have a broad foundation.

I omit any special reference to the Archæan elevations, and also the local disturbances in the Primordial of Newfoundland, as well as the facts relating to minor changes of level.

A. Mountain-making on the Atlantic border.

(1.) At the close of the Lower Silurian, or a little earlier, a culmination of the great Appalachian geosynclinal resulted in displacements, plications and metamorphism, and the making of a *synclinorium*, along the *Green Mountain* region—these mountains (some summits at present over 4,000 feet high above the sea) being the result. The depth to which the region subsided during the Lower Silurian era, and the thickness of the accumulations, are not ascertained; probably the extent was not less than 20,000 feet.

(2.) Simultaneously, a *permanent anticlinorium* was made over the Cincinnati region, from Lake Erie into Tennessee, parallel with the Alleghanies of Virginia, 250 miles to the northwest.

(3.) The Acadian region—embracing western Newfoundland, St. Lawrence Bay, the Bay of Fundy, and part of Nova Scotia and New Brunswick adjoining, and probably the sea southwest between St. George's Bank and the coast of Maine, with also an area in Rhode Island—was the course of a great geosynclinal, or a series of them, parallel in general direction with that of the Appalachian region; it continued in progress, but with mountain-making interruptions, and some shift of position to the eastward, from the Silurian to the close of the Jurassic.

At the close of the Lower Silurian, no general disturbances occurred in this Acadian region, so far as is known. In the Anticosti seas, or northern part of St. Lawrence Bay, lime-

stones, as Logan states, were uninterruptedly in formation from the beginning of the Hudson period of the Lower Silurian to the middle of the Upper Silurian, showing that the Acadian geosynclinal was then in regular progress. It so continued until—

The close of the Devonian, when disturbances, plication and metamorphism took place in eastern Canada, Nova Scotia and the bordering region of New Brunswick, and the most extensive of Acadian Paleozoic synclinoria resulted, according to the observations of Dr. Dawson and others.

(4.) The close of the Carboniferous age was an epoch of mountain-making in the Alleghany region, the Alleghanies from New York to Alabama having been then made, as already explained.

(5.) At the same time there were disturbances and synclinorian plications in the Acadian region. During the Carboniferous era, according to Logan and Dawson, 16,000 feet of rock had in some parts accumulated, and therefore a geosynclinal of 16,000 feet formed, the rocks in their many coal-beds and root-bearing layers bearing evidence, to the last, of oscillations involving an intermittent but progressing subsidence. The synclinorium, the resultant, was much less marked than that at the close of the Devonian.

(6.) During the Paleozoic, along the sea-border, a more or less perfect barrier was made by a geanticlinal uplift (anticlinorian), which was a counterpart to the geosynclinal of the Appalachian region. (See beyond.)*

(7.) The middle or close of the Jurassic period was an epoch of displacements, and the making of a series of imperfect synclinoria along the Triassico-Jurassic areas from Nova Scotia to Southern North Carolina, as sufficiently described.

(8.) During the era of the Connecticut River sandstone (Triassico-Jurassic) a nearly complete sea-border anticlinorium existed—a counterpart to the progressing geosynclinal. Its existence is proved by the absence of all marine fossils from the beds.*

(9.) The era closing the Cretaceous, and that of the Tertiary, witnessed but small uplifting and some local displacements of the rocks of these eras on the Atlantic border. The principal movement was geanticlinal, and it involved probably the whole Alleghany region.

(10.) In the Quaternary there were extended movements of geanticlinal and geosynclinal character which need not be here described.

* In my Manual of Geology, the probable existence of such a barrier is recognized in connection with the remarks on the geography of the Trenton period in America; and it is particularly dwelt upon, and illustrated by a map, in the chapter on the Triassic; but it is not spoken of as connected in origin with the geosynclinal that was in progress to the west of it. Evidence with regard to this anticlinorium is given in the following part of this memoir.

B. Mountain-making after Archæan time on the Pacific border, within the territory of the United States.

(1.) At the close of the Lower Silurian, none yet known.

(2.) At the close of the Devonian, none yet known.

(3.) At the close of the Carboniferous age, or the Paleozoic, none yet known; and if none really occurred, then the contracting globe at that time, as far as U. S. N. America is concerned, must have expended its energies, which it had been gathering during the Paleozoic, in making the Alleghanies and in some minor plications along the Acadian region

The "Great Basin," between the Sierra Nevada on its western border, and the Wahsatch range on its eastern (lying along the meridian just east of the Great Salt Lake), contains a number of short ridges, parallel to these lofty border ranges, some of which are quite high;* and they consist, according to King, "of folds of the infra-Jurassic rocks"; and "it is common to find no rocks higher than the Carboniferous," owing, it is stated, to the erosion that has taken place. It is not clear that part, at least, of the Great Basin plications may not have taken place before the Jurassic era. If not, then the movements must have been in some way involved with those of the Sierra and Wahsatch regions.

(4.) At the close of the Jurassic, two great geosynclinals, which had been in progress through the Paleozoic and until this epoch in the Mesozoic, culminated each in the making of a lofty synclinorium—one, the Sierra Nevada, some of whose summits are over 14,000 feet high; the other the high Wahsatch, a parallel north and south range.

Whitney has proved that the Carboniferous and Jurassic rocks are conformable in the Sierra Nevada range, and that the close of the Jurassic was the epoch of its origin; but direct proof is not yet found that the Devonian and Silurian formations are included. The granite axis of the chain probably indicates, as LeConte has suggested, the region of maximum disturbance and metamorphism.

The Wahsatch contains, according to Clarence King, formations of all the ages from the Lower Silurian to the Jurassic, and the whole are throughout conformable; and a great thickness of crystalline rocks exists beneath, supposed to be Archæan, which he states are conformable also. The plications and mountain-making took place, as King states, cotemporane-

* An admirable chart, giving in detail the topography of this whole region, and including the Wahsatch, has been prepared by Mr. James T. Gardner after careful surveys by himself, topographical surveyor of the Exploration of the Fortieth Parallel under Clarence King, and is now ready for the engraver. Mr. King has published thus far only brief chapters on the geological results of his survey, in the volume of J. T. Hague on Mining Industry (vol. III). He has ready for publication Vols. I and II, on Systematic and Descriptive Geology. The Botanical Report of the Survey, Vol. V, has been issued; but Vol. IV, on Zoölogy and Paleontology, remains to be completed.

ously with the same in the case of the Sierra, before the Cretaceous era, the Cretaceous beds lying on the Jurassic unconformably.

These two synclinoria are 400 miles apart. The preparatory geosynclinal of the Wahsatch—and probably that of the Sierra—took for its completion, supposing it to have begun with the opening Silurian, a period at least a fifth longer than the whole Paleozoic.

(5.) At the close of the Cretaceous, another pair of geosynclinals, parallel with the coast, but geosynclinals of only Cretaceous origin, culminated in synclinoria.

One of the Cretaceous geosynclinals was in progress *east of the Wahsatch*, along the whole summit region of the Rocky Mountains, in the United States. Directly east of the Wahsatch, according to King, the beds are 9,000 feet or more thick; and, as Hayden states, they have a great thickness in the Laramie Plains, and little less over the upper Missouri region; so that the downward movement was in some parts a profound one, and affected a very wide extent of country. Hayden and King make this disturbance to have taken place after part, or all, of the Eocene period had passed, while Prof. Marsh holds that it occurred at the close of the Cretaceous period.*

* Clarence King has very briefly described the Wahsatch region, as well as the country to the west, in the third volume (4to, 1870) of his United States Geological Exploration of the 40th parallel; and on page 454, he says: "Subsequent to the laying down of the old Cretaceous system, and of those conformable freshwater beds which close the coal-bearing period, another era of mountain uplifts occurred, folding the coal series [Cretaceous and Lower Tertiary] into broad undulating ridges having a general trend of northeast." He then observes that freshwater Tertiary beds of sand and clay, "an immense accumulation," were laid down *unconformably* over this upturned Cretaceous, and, after the Miocene era, were subjected to "orographic" disturbance and "tilted to an angle of 15° to 20°, or thrown into broad and gentle undulations wherever they lie in the neighborhood of the older ranges such as the Wahsatch and Uintah." These disturbances were confined to within 15 miles of the Wahsatch. The period in which they occurred witnessed also great outflows of trachytic rocks in this and other parts of the Rocky Mountain region. Mr. King adds, on page 455, that there is no question as to the identity of the beds that *overlie unconformably* the Cretaceous folds along the eastern flank of the Wahsatch with the horizontal Tertiary deposits of the Green River Basin; and that over this basin between the Green River and the Wahsatch, no single instance of conformity occurs between the coal beds and the overlying horizontal freshwater strata. As stated above, he makes the epoch of Cretaceous uplifting to have followed, not the Cretaceous period, but the earliest period of the Tertiary, Eocene beds being, in his view, included with the Cretaceous in the folds referred to.

Dr. Hayden has investigated with much detail the Green River Basin and the region east of it, and years since announced that the Lower Tertiary beds, in some parts of the Rocky Mountain region, were tilted at a high angle. He has held that all the Coal-bearing strata were Lower Tertiary; but now agrees with the view expressed by King, and first suggested by Meek, that part are Cretaceous, while another part are Lower Tertiary, and considers the later Tertiary beds, which lie unconformably on the beds below in the regions of disturbance, Miocene and Pliocene. He states that the thickness of the Cretaceous formation in the Laramie Plains is 8,000 to 10,000 feet. He observes in a recent letter to the writer that the Coal-bearing strata and Cretaceous are never unconformable; but

The other geosynclinal belt of the Cretaceous era was to the *west of the Sierra Nevada*, as described by Whitney. This coast geosynclinal ended in extensive displacements and plications, much metamorphism, and a high synclinorium.

(6.) The intermediate region—the Great Basin, which had been widened at the close of the Jurassic by the annexation of the plicated and consolidated Sierra and Wahsatch—was the area of a geanticlinal, or at least of absence of subsidence; for King says no Cretaceous rocks occur over it.

(7). With the close of the Cretaceous, or when the Cretaceous synclinorian movements of the sea-coast and mountains were ending, a geanticlinal movement of the whole Rocky Mountain region began, which put it above the sea-level, where it has since remained. This upward movement continued through the Tertiary.

(8.) During the Tertiary age, until the close, probably, of the Miocene Tertiary, another pair of parallel geosynclinals—but geosynclinals of Tertiary formation—were in progress. The Cretaceous synclinoria had given still greater breadth and stability to the relatively stable region between them, and one of these new troughs is hence farther east on the mountain side, and the other farther west on the coast side.

In the coast geosynclinal, marine Tertiary beds were accumulated to a thickness of 4000 to 5000 feet; and then followed the epoch of disturbance ending in another coast synclinorium, a coast range of mountains, in some places metamorphic, and having ridges, many of which are at present 2,000 feet or more in height above the sea, and some in the Santa Cruz Range, according to Whitney, over 3,500 feet.

The other is to the east of the Cretaceous axis in the summit region of the Rocky Mountain chain. A great thickness of freshwater beds was made in the Green River region and some other places about the Rocky Mountain summits, and thinner deposits to the eastward. The thickness, in connection with

instead are often folded together, and sometimes stand at a high angle, even vertical in many places, as in the Laramie Plains south of Fort Sanders; along the Big Horn region; between Long's Peak and Pike's Peak; near Denver in Colorado, etc. Near the mouth of the Big Horn. The Chetish or Wolf Mts. consist of these upturned strata and have a height of 1500 to 2000 feet above the Yellowstone. He found the later Tertiary beds sometimes tilted at a small angle, never over 10°.

The discovery of Dinosaurian remains in some of the Coal beds, announced by Marsh and Cope, and of *Inocerami*, as ascertained by Meek, is one part of the evidence on which the lower parts of the Coal beds is determined to be Cretaceous. Besides this, there is the fact that the supposed Miocene of the Green River Basin contains remains of mammals that are decidedly Eocene in character; and if these are Eocene, then the Coal beds are something older. Prof. Marsh is very strongly of the opinion that all the *Coal beds are Cretaceous.*

On the other side, Lesquereux states that the evidence from fossil plants is totally opposed to making any of the Coal strata Cretaceous.

The method of mountain-making, and the principle involved, are the same whatever be the decision as to the exact epoch of the Cretaceous plication.

evidences of shallow water origin, indicates a progressing geosynclinal, although the ocean gained no entrance to it. The downward bending ended probably just after the Miocene period without general displacements; but there were tiltings along the more western border of the Tertiary in the vicinity of the Wahsatch and other mountains. (See note on page 439.)

(9.) Since the Miocene era, and on through much of the Quaternary, there have been vast fissure-eruptions over the western Rocky Mountain slopes. They had great extent especially in the Snake River region where the successive outflows made a stratum 700 to 1000 feet thick, over an area 300 miles in breadth. There are other similar regions but of less area.

It is thus seen that along the Pacific side of the continent the crust, under the action of lateral pressure, first bent downward profoundly, and then yielded and suffered fracture and plications, directly along a belt, parallel with the coast, either side of the Great Basin (and perhaps over this basin to some extent), the two great lines 400 miles apart. The plicated regions, thus made, having become firm by the continued pressure and the engendered heat and resultant solidification, the crust next bent, and then yielded, in a similar way, along an axis outside of the former regions of disturbance, the two axes over 600 miles apart; and again all was mended in the same way. Then it bent a third time, just outside of the last range, on each side of the same great area, the lines over 700 miles apart; and then, over the western of the two ranges, the beds were displaced, solidified, and left in high ridges; but over the eastern the final disturbances were local and slight.

There were hence two parallel series, cotemporaneous in steps of progress, situated on opposite borders of the Great Basin, a *coast* series, and a *mountain* series, each having its highest member toward the basin; the coast series, the grandest in its three parts, and leaving evidences of the profoundest disturbance, and the greatest amount of metamorphism. The Wahsatch range is nearly as high as the Sierra; but probably a fourth of its height is due to the final elevation of the Rocky Mountain region.

The last bendings were more local than the preceding because the crust had become stiffened by its plicated and solidified, and partly crystallized, coatings, as well as by thickening beneath; and therefore, while the Tertiary movements were in progress, the part of the force not expended in producing them carried forward an upward bend, or geanticlinal, of the vast Rocky Mountain region as a whole. For the same reason, profound *breakings* took place where bending was not possible, and thereby immense floods of liquid rock were poured out over the surface. (Most of the great mountains of the globe were lifted about this time, that is, in the course of the Tertiary era, and many of the great volcanoes were made.)

There were irregularities or exceptional courses in connection with this system of movements and their effects. But these show only that in the same area the lateral pressure at work was not alike either in amount, or in direction, in different latitudes; nor was the resistance before it the same.

The results correspond with the well-understood effects of lateral pressure. Suppose a long beam, having an even texture except that a portion toward the middle (say a sixth of the whole length) is stouter than the rest, to be subjected at its extremities to direct pressure. The first yielding and fracture would take place toward the stouter portion on either side. If this break were mended by splicing and cementing until firmer than before, the next region of yielding would be just outside of the former. In brief, the fracturing would be in each case near the stouter portion of the beam. Moreover, the extent of the yielding and fracture on each side would have some relation to the amount of pressure against that side. Just so has it been with the earth's crust under the action of lateral pressure. The facts further illustrate the truth, before announced, that the force from the ocean side had in some way the advantage, and in fact was the greater. But the full difference is not indicated by the difference in the results of disturbance, since the shoving force on the side of greatest pressure would not be limited in its action to its own side, unless the intermediate stouter region were wholly immovable.

C. Movements over the Oceanic areas.

The history of the changes of level over the oceanic areas is necessarily a meager branch of geological science. There are, however, some great truths to be gathered which are of profounder import than is generally acknowledged. They show that the occeanic crust has sometimes acted in the capacity of a single area of depression, although of so immense extent. I allude briefly here to only two of the facts, referring the reader to my former articles for a fuller discussion of the subject.

First, the remarkable one that nearly all the ranges of islands over the Pacific ocean, and even the longer diameters of the particular islands, lie nearly parallel with the great mountain ranges of the Pacific coast of North America. There is a dynamical announcement in this arrangement—which is partly recognized when we refer it, as I have proposed, to the existence of directions of easiest fracture in the very nature of the infra-Archæan crust, and regard the courses of these feature lines of the oceans and continents as having reference to one of these directions. But, besides this, there is a declaration with regard to the *direction* of the pressure that acted against the continents and reacted over the oceanic areas.

The other fact is that of the Coral island subsidence, already referred to, which affected the tropical ocean for its whole

breadth, or more than a quarter of the circumference of the globe; sinking the sea bottom at least 3000 feet over a large part of the area, and probably over its axial portions two or three times this amount.* The oceanic basin was evidently one basin in its movement; but the areas of less and greater subsidence, of parallel N.W. by W. trend, so alternate along the southern border of the region of subsidence that we may conclude there were great parallel waves, made by lateral pressure in the crust, as I have elsewhere explained,† that is, geosynclinals and geanticlinals, such as are the only possible conditions of the crust under the lateral pressure of contraction. Now this great oceanic subsidence, involving the breadth of the ocean, if begun in the Tertiary era, as is probable, was going forward at the very time when the Rocky Mountains, and other great mountains of the globe, were in progress of elevation, as if these were counterpart movements in the earth's surface; and it continued on during the Glacial era, when the continental elevations appear to have reached their highest limit.

We gather from these facts how it is that a general submergence, or an emergence, might characterize cotemporaneously large areas of North America and Europe; as, for example, in the Subcarboniferous, Carboniferous and Permian periods, during which the rocks show that there was a general parallelism in the movements. If a geanticlinal were in progress over the middle of the Atlantic crust, as a result of the lateral thrust in the continental and oceanic crusts, there might also be a reverse movement or general sinking along the continental borders, as well as a rise of water about the continents from the diminution in the ocean's depth; and when the oceanic geanticlinal flattened out again through subsidence, the subsiding crust would naturally produce a reverse movement along one or both continental borders.

From the various considerations here presented, derived from both the continental and oceanic areas, it is apparent that the earth has exhibited its oneness of individuality in nothing more fundamentally and completely than in the heavings of its contracting crust.

The subjects of metamorphism, the earth's interior, igneous eruptions and volcanoes remain for discussion. In addition I propose to consider the steps in the origination of the continental plateaus and oceanic basins, and also present some facts bearing on the general nature of the infra-Archæan crust, that is, the part below the earth's superficial coatings.

* Author's Rep. Geol. Wilkes U. S. Expl. Exped., 4to, 1849, p. 399; and Corals and Coral Islands, 8vo, 1872, p. 329.

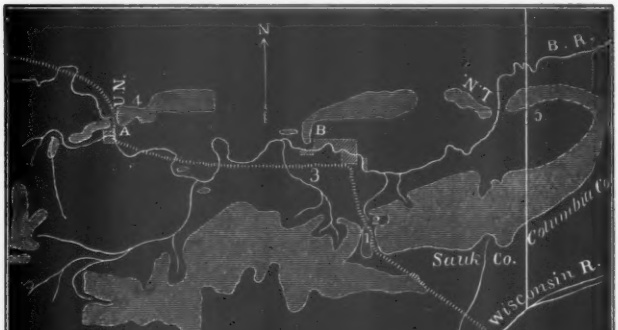
† Rep. Geol. Expl. Exped., p. 399; Corals and Coral Islands, p. 328.

ART. XLVII.—*On the relations of the Sandstone, Conglomerates and Limestone of Sauk County, Wisconsin to each other and to the Azoic;* by Prof. JAMES H. EATON.

THE age of the quartzite hills and ridges of Sauk County has been satisfactorily determined by Mr. Roland Irving* to be Pre-silurian. Mr. James Hall† in his report of the State Survey calls them Huronian. On Dr. Lapham's map a small region on the Eau Claire River, adjacent to the great central area of granitic rocks, is colored as quartzite. An examination of this locality, to determine whether the latter rest unconformably upon the former, would perhaps determine their age. For the present we can say that these rocks differ lithologically.

The accompanying map is by Mr. Wm. H. Canfield, of Baraboo, who for many years has been the officialy surveor

1.



A, Abelman; B, Baraboo; B R, Baraboo River; L N, Lower Narrows; U N, Upper Narrows. 1, Devil's Lake; 2, 3, Potsdam Sandstone; 4, Section; 5, Limestone. Scale, three-twentieths of an inch equal to a mile.

for Sauk County, and it is taken from surveys made by him with the especial purpose of marking the quartzite outcrop. It has been completed for Columbia County by Mr. T. C. Chamberlain, of Whitewater. The dotted line east of the Upper Narrows was also added by Mr. Chamberlain. It is believed that this map shows the entire outcrop of Azoic rocks in the region of the Baraboo River.

We have thus represented a group of islands which existed in the Potsdam sea, with their common trend east and west, or at right angles to the dip of the rocks.

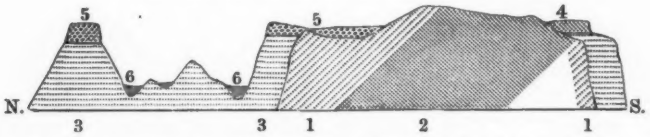
* This Journ., Feb., 1872. † Survey of Wis., p. 11.

The following localities were visited by myself in the fall of 1872:

The point marked (2) on the map is the locality from which the fossils were obtained, which were described by Mr. Alexander Winchell * in 1864. In a short time a large number of fossils were obtained from loose pieces of sandstone. I was assured that there is a quarry of the rock in a place near by, but the time did not suffice to find it. The fossils were *Scolithus linearis* Hall, *Orthis Barabuensis* Winchell, *Straparollus primordialis* Winchell, *Dicellocephalus Minnesotensis* Owen.

At the locality marked (4), however, is a section (fig. 2) which is truly magnificent in its exposure, of all the rocks which belong to this region, in their stratigraphical relations, except the limestone. It is at the railroad station, Abelman. The Baraboo River, in forming the Upper Narrows, has left upon the

2.—Section at Upper Narrows, Baraboo River.



1, Quartzite. 2, Metamorphic Conglomerate, 3, Potsdam Sandstone- 4, 5, Conglomerates. 6, Drift.

east side a nearly vertical section, about half a mile long and 300 hundred feet high at the highest point. This section is of a core of tilted rock, flanked on *both* sides by horizontal Potsdam sandstone and conglomerates. No doubt can therefore remain that the tilted rock is Pre-silurian.

The dip of the entire section of Azoic rock is to the north or slightly west of north. Its face is cut by numerous vertical joints in the same manner as the cliffs at Devil's Lake. At the extreme southern end the rock varies from a compact dark-colored homogeneous quartzite, to a much less compact and lighter quartzite. One large detached block of the hard, dark quartzite was seen beautifully covered with ripple-marks.

Passing along the face of the cliff toward the north it becomes covered with large blocks of quartzite, sandstone and conglomerate, which have fallen from above.

Coming to the exposed rock again, it changes to a metamorphic conglomerate. This makes up more than half the section. It consists of *angular* pieces of the compact Sauk quartzite, firmly imbedded in a cement of white crystalline quartz. The former vary in size from small fragments to

* This Journ. II, vol. xxxvii, p. 226.

masses several tons in weight. Numerous cavities are lined with quartz crystals. The dip here is from 75–80° N.

The remainder of the section consists of the homogeneous, dark, compact quartzite, bedded in the same manner. We have then indications of three successive sets of circumstances in Azoic times; those in which were formed respectively the underlying quartzite, the conglomerate and the overlying quartzite. The lower quartzite must have been already hardened from the moving sands before it was broken into fragments for the conglomerate. And then its cement was crystallized. Finally, layers of sand spread over this were hardened.

As has been said, upon the southern flank of this Azoic core, horizontal beds of Potsdam sandstone lie unconformably. They extend a short distance over the edges of the upturned beds of quartzite.

The relation of this sandstone to the underlying quartzite shows most unmistakably the effects of shore action. The quartzite is generally in place, but the large blocks, formed by the crossing of the planes of bedding and the joints, are somewhat isolated, as if they had formed crags on an old coast, where the wearing of the waves had enlarged the cracks. Into these fissures and crevices the sand is forced. There are also blocks of quartzite, that have been displaced somewhat, which are enveloped in sandstone. In the sandstone itself is an occasional rounded pebble of quartzite. The sandstone which rests upon the northern flank is irregularly bedded, having the ebb and flow structure. Farther north are three isolated hills of sandstone.

Resting on the sandstone at the south, and stretching also over the quartzite, is a conglomerate made up of a friable sandstone, like that below, containing numerous rounded pebbles of the quartzite of various sizes. The cement makes up a considerable part of the rock. This conglomerate, as I have assured myself by careful examination, is exactly like that mentioned by Mr. Irving as occurring on the quartzite just northeast of Devil's Lake, and containing Potsdam fossils. The finding of this conglomerate, therefore, in its true relation, verifies Mr. Irving's supposition in opposition to Mr. Winchell, that neither the conglomerate nor the quartzite is the *base* of the Potsdam system, for here the true base comes in between, as sandstone. In the same manner there is a conglomerate at the north resting conformably on the sandstone and unconformably on the quartzite. One of the isolated hills of sandstone is also capped by the same. As nearly as could be determined, the level is the same as that of the conglomerate at the south. But its character is different. It is made up almost entirely of small rounded pebbles of quartzite of a pretty uniform size. The cement is quite hard, but true sandstone.

This section then represents an old Azoic reef of tilted rock, running east and west, washed upon either side by the waves of the Potsdam sea. On the south the action appears to have been gentler than on the north, for while at the south the quartzite has been triturated to a fine sand, containing, to be sure, larger or smaller pieces of quartzite, well rounded, the northern shore must have been exposed to the breakers which washed out the fine sand and left pebbles of a uniform size. It may be that within the circle of these islands was a sheltered bay. Mr. Chamberlain has observed at a little distance back from the edge of the cliff, sandstone again covering the conglomerate and, in fact, the entire length of the quartzite, indicating a subsequent subsidence of the entire reef below the water.

At the point marked (5) on the map is a limestone quarry. It is horizontally bedded. All points of junction with the underlying rock are concealed, but it is plainly, at least, 100 feet below the Potsdam sandstone in place. Whether it is a local deposit in the Potsdam sandstone or is the Lower Magnesian limestone, I have not yet determined. The latter supposition requires an enormous erosion between the putting down of the Potsdam sandstone and the Lower Magnesian limestone. A number of fossils were secured, several cephalic shields of a trilobite, a *Pleurotomaria?* and others still more indefinitely known.

Another feature of interest in this region is the evidence of glacial action aside from the drift. At the point (3) on the map is an isolated hill of sandstone. On my visit the earth had just been removed from a large surface in order to quarry the rock. It was entirely smoothed and covered with glacial striæ. Their direction was N. 66° E. On the surface of the limestone previously mentioned, the polishing is even more perfect, and the striæ have the same direction. The only way I can explain this deflection from the usual direction is, that it was caused by the trend of the ridges. At the Glacial epoch, the erosion of the Baraboo Valley must have been as great as at present.

Mr. Canfield writes me: "The polished surface of quartzite that I especially mentioned to you, * * * showing probable glacial rubbing, was three-fourths of the way up the bluff in section 27. I also stated to you my convictions that the evidences of that character could be found all over the tops of the bluffs, within the glacial limit, which is east of the 'Lake of the Bluffs.' There is certainly a great deal of granitic debris piled upon the top of the bluffs."

Beloit, Wisconsin, Feb. 14, 1873.

ART. XLVIII.—*On the formation of the features of the Earth-surface. Reply to criticisms of T. Sterry Hunt;* by JOSEPH LECONTE, Prof. Geol. Univ. of California.

IN the April number of this Journal, p. 264, Prof. Hunt reviews my paper "On the formation of the great features of the earth-surface,"* criticising *some* points and making reclamation of *others* for himself. In his criticisms he has sometimes misunderstood and sometimes, I think, hardly fairly represented me. In his reclamations, it seems to me, that, in his anxiety to press *yet once more* upon the attention of geologists his own labors, he has mistaken the *use of similar* materials for the *similar use* of materials. That I have used materials similar to those used by himself and many others, I admit; but I have used them certainly in a different way and for a different purpose. Before I commence to show this, however, let me at once acknowledge my deep indebtedness to Mr. Hunt for the many valuable suggestions which I have gotten from the thoughtful and repeated study of his numerous papers. I frankly confess that it was the study of these that first turned my attention to what I now regard as the true *starting point*, and for this I most heartily thank him; but from this starting point we have certainly worked in somewhat different directions, as I now proceed to show.

1. My starting point is the proposition that "the whole foundation of theoretic geology must be reconstructed on the basis of a solid earth." But Mr. Hunt states that this has been his starting point ever since 1858. This is very true. I did not say it was a *new* starting point. I am sure no reader of my paper could imagine that I made any such claim. On the contrary, this starting point is necessitated by a belief in the solidity of the earth, to which belief, I had just previously stated (p. 352), many of the most advanced geologists were fast coming. Among these advanced geologists, I had in my mind Mr. Hunt, but did not think it necessary to mention so obvious a fact. I am sorry, now, I did not If I deserve any credit at all in connection with this much coveted starting point, it is in collecting and arranging the evidence against the *other* starting point, and in favor of this one, and thus justifying my selection as the only rational one.

2. Again: after leaving the starting point, one of the important steps in my march was *the aqueo-igneous fusion of deeply buried sediments and their consequent metamorphism by the rise of the geo-isotherms.* But Mr. Hunt claims that this, too, he has

* This Journal, vol. iv, p. 345 and p. 460.

been insisting upon for years. Very true, again. I suppose there is no geologist in this country, or in Europe, who is not fully aware of this. Mr. Hunt does not give sufficient credit to the good memory of geologists, or to the importance of his own papers, or he would not have thought it necessary to mention it again. If the idea had been original with Mr. Hunt, and confined to him, it would have been necessary to credit it to him; but it had already become the common property of science; it had been used by Herschel, and Scrope, and Lyell, as well as by Hunt and Hall. Surely, I did not claim originality, for I spoke of it as a well known *recent* view (p. 467); my only fault, then, was not mentioning Mr. Hunt. I am sorry, again, I did not do so, but I can assure him it was only because I regarded his writings as so well known.

3. Again, Mr. Hunt had stated in one of his early papers that the aqueo-igneous fusion of deeply buried sediments "offers a ready explanation of the phenomena of volcanoes and igneous rocks," and he now states that my views are similar to his. Mr. Hunt, it seems to me, has speculated somewhat vaguely on this subject. Sometimes he speaks of a *zone* or *region* of plastic matter separating a solid nucleus from a solid crust (p. 264), through which zone the shrinking nucleus would act on the solid crust precisely as if the whole interior were liquid; sometimes he speaks more truly of *local* masses of plastic matter beneath great *recent* accumulations of sediments. The generation of gases and vapors within the sediments is, according to him, the *force* of eruption. Now, according to my view, a zone of liquid matter would not do at all: the liquid mass is local, and is *squeezed out* by horizontal pressure through fissures, as great *massive eruptions*, often forming the great mass of mountain chains, and must be *local* in order that it should be squeezed out. Volcanoes, I suppose, are parasites on these great out-squeezed masses, produced by the access of water to their still hot interior. Let it be remembered, however, that my subject was a theory of *mountain chains;* my principal object, therefore, was not a general theory of volcanoes, but to explain the *association of both massive eruptions and volcanoes with mountain chains.*

4. I had stated (p. 468) that the aqueo-igneous fusion of sediments determined lines of weakness and, therefore, lines of yielding to horizontal pressure. Mr. Hunt now states that he had previously expressed similar views in similar language. I freely admit this, although I did not know it when I wrote; it had escaped my memory. This is the only sin I have to acknowledge. As far as I know, the idea is original with Mr. Hunt, and I ought to have known it and credited him with it. I now do so most heartily. But observe, my main object was

not the fact itself, but the *use* of the fact in sustaining my theory. All I claim here, therefore, is the connection of this fact with the *position and formation of mountain chains.*

5. I attribute the enormous *foldings* of the strata of mountain chains to horizontal crushing together, produced by the interior contraction of the earth. Mr. Hunt makes reclamation of this also. Let us compare our views on this subject. Mr. Hunt attributes the folding of the Appalachian chain to three causes; (*a*) Subsidence of a convex mass of sediments; (this I have shown (p. 461) could not take place if the sedimentation and the subsidence went on *pari passu.* (*b*) Contraction of the strata by metamorphism; (this I suppose could only produce foldings by producing subsidence of the convex surface, and therefore, is subject to the same difficulties as the last.) (*c*) Horizontal pressure produced by interior contraction. The first, Mr. Hunt says, is probably not the *principal* cause, but I cannot think he regarded the last as a great cause, for *he does not connect it with elevation of the chain.* Now, according to my view, *folding is produced entirely by the horizontal crushing; and indissolubly connected with this crushing together horizontally, is the up-swelling vertically, and the formation of the mountain chain.* Again, Mr. Hunt's Appalachian foldings were going on during the whole process of sedimentation; mine commenced only after the sedimentation was completed. That the latter view is the true one, is proved by the fact that the last layer of the Coal-measures is not only folded, but folded equally with the lower strata. That the folding took place only after sedimentation was completed, is not, of course, original with me: it is the common belief of geologists. All I claim is the *connection of foldings, on the one hand, with horizontal crushing as a sole cause, and on the other with the formation of the chain.*

6. I stated (p. 462), that Hunt and Hall leave the sediments just after the whole preparation had been made, but before the actual mountain formation has taken place, and, therefore, this theory had been characterized as a theory of mountains with the mountains left out. Mr. Hunt, referring to this statement, says that Hall and himself had been much misunderstood by myself as well as by others; that neither he nor Hall ever proposed any theory of mountain formations at all, but only a return to the views of Buffon and Montlosier, that "mountains are fragments of denuded continents."

In order to make my explanation of this point clear, I find it necessary to define my terms. The word *mountain* is *loosely* used, in scientific as well as in popular language, to express every considerable inequality of the earth surface, from a great mountain chain like the Andes or the Himalayas, to mere hills of circumdenudation like those on the upper Mississippi. The

result has been much confusion of thought. For it is evident that the great bulge which constitutes a mountain chain, and which can be seen only from a distance, is formed in an entirely different way from the smaller inequalities which constitute scenery; the former is evidently produced by general causes affecting the whole earth, the latter wholly by *erosion*. I think therefore, that it is necessary very carefully to distinguish these in our theories. In my own lectures I no longer divide mountains into two kinds, mountains of *upheaval* and mountains of *erosion*, but simply treat the whole subject of mountains under the two heads of *mountain formation* and *mountain sculpture*. All portions of continents, it is true, are sculptured in this way, but this is especially true of mountain chains, which are the great theaters of erosion as of igneous agencies. When I speak of mountain formation, therefore, I mean only the formation of the great bulge or convex plateau which constitutes the *chain;* but when Mr. Hunt speaks of mountains as "fragments of denuded continents," he refers, of course, not to the chain, but to the smaller inequalities, or the effects of *sculpture*. It is certainly one of the great glories of American geology, to have *clearly* shown by the study of the Appalachian chain the immensity of this work of erosion; that not only the smaller ridges and ravines, but great cañons, wide valleys and lofty peaks owe their origin to this cause alone. To Lesley, Hall and Hunt is chiefly due the credit of expounding these views. I confess their writings have been of immense service to me in my mountain studies. But I insist that a theory of these is not a theory of *mountain chains*. The older geologists, it is true, neglected far too much the effects of erosion, and attributed every peak, and ridge, and valley, to upheaval, or fracture, or engulfment; but there still remains the great bulge or convex plateau, the real chain, to be accounted for; for no one imagines *this* to be the result of erosion.

Now it is precisely this convex plateau which, I had supposed, Hall and Hunt attributed to *sedimentation*. I had supposed that they regarded the Appalachian chain as first a great *convex submarine plateau* produced by a line of sedimentation; then this raised into a *convex mountain plateau* by continental elevation; and finally this plateau sculptured into its present forms. I admit that all of this is not clearly and definitely expressed, but when I attempted to formulate their views clearly I could arrive at no other result. Others, I believe, have arrived at a similar interpretation of their views. Under the impression that this was their view, I proceeded to show that it certainly was not true, since the Appalachian region at the end of the Paleozoic era, and immediately before the formation of the chain, was not a *convex* plateau of any kind, but a *concave*

trough, and the whole bulging took place afterward by the crushing together of its strata. But now (if I understand him aright, for he is still not very clear) Mr. Hunt says that the Appalachian plateau or chain was formed by the same unknown process by which the continent was elevated; that it was formed by continental elevation, which from some unknown cause *was greater in the Appalachian region.* I wish much he had clearly expressed this at first; it would have saved much useless discussion. I confess, however, I can not find anything like this in his previous papers or in the writings of Prof. Hall. In the early presentation of a difficult subject, however, some want of clearness is pardonable.

7. According to my view, *foldings* are a necessary concomitant of mountain formation; but Mr. Hunt, p. 267, thinks both cleavage and foldings are mere *accidents*, unnecessary to mountain structure; and he cites examples of mountains on the upper Mississippi composed of *perfectly horizontal strata*, and of Catskill mountain composed of *nearly* horizontal strata, *uncomplicated with foldings.* I could add other examples from my own observations on the Sierra chain. Mt. Dana, a magnificent peak more than 13,300 feet high, on the very crest of the Sierras, is composed of strata which seem to be perfectly horizontal. But this is no objection to my theory; it is only an example of the confusion of thought of which I speak above. The explanation of the difference between mountain *formation* and mountain *sculpture*, between mountain chains and so-called erosion mountains, completely answers this objection. I was speaking of *chains*, not of isolated *peaks.* Mountain chains are, I believe, always folded; but in the extensive erosion of the *wide* folds of thick strata it would be strange indeed if *no* portions where the strata were horizontal were left as peaks. This is the case, doubtless, with Catskill and Mt. Dana. As for the erosion hills of the upper Mississippi, the explanation is still simpler. There is no mountain chain there at all; these hills are only fragments of denuded continental strata.

8. I hold the view (p. 464) that during the Paleozoic era there existed a continental mass, probably of considerable extent, to the eastward of the Appalachian region. Mr. Hunt again makes reclamation for himself and others. Surely there was no necessity for reclamation. If I had imagined that this was new, I would have devoted a whole paper to it, not have dismissed it in a single very short paragraph. But I certainly do not leave this point doubtful; on the contrary, I say "evidences are daily accumulating" on this point; not by *my* labors; for I was on the Pacific coast; but by the labors of others. I of course referred to the very evidence which Mr. Hunt mentions, but did not think it necessary to mention names in connection with facts so

well known. If I deserve any credit in this connection, it is in giving *something more of definiteness to the conception* and *especially in showing its connection with the formation of the Appalachian chain.*

9. In the last paragraph of my paper, I acknowledge my inability to explain those "great and wide-spread *oscillations* which have marked the great divisions of *time*, and have left their impress in the general unconformability of the strata; the last being that of the Post-tertiary period." After quoting this, Mr. Hunt goes on to say that it is precisely this *upward* movement which *constitutes the continental elevation of* Montlosier, Hall and himself, and *which give rise to plateaus* by the erosion of which are formed mountains. And since I regard plateau-formation the only true mountain formation, by my own admission (so argues Mr. Hunt) mountain formation is still unexplained. Thus Mr. Hunt makes me, after writing a paper to explain the formation of mountain chains, in the last paragraph acknowledge that it is inexplicable. I wonder Mr. Hunt did not rather suspect that he had entirely misunderstood me.

It was not *upward* movement or *downward* movement which I regarded as inexplicable, but *oscillation* or movement *upward and downward alternately in the same place.* All the causes of movement of which I had previously spoken would *continue to act in the same direction*, and therefore the continents ought to grow higher and larger and the sea-bottoms deeper. On the whole, this has probably been the case throughout the geological history of the earth, as has been so beautifully shown by Dana for the North American continent. The recent observations on the wonderful *persistence of deep-sea faunæ and therefore of deep-sea conditions*, through many geological periods, go far to confirm this view for the sea-bottoms. Previously geologists had mostly regarded the earth's crust only as *oscillating;* Dana showed *gradual development* or evolution of continents in the midst of oscillation. Now I have attempted to explain the *development*, but acknowledge my inability yet to explain the *oscillations.*

10. Finally, Mr. Hunt criticises, and perhaps justly, my views as to the chemical cause of the intense heat so often found in lavas. Since the publication of Mr. Mallet's paper, I much prefer adopting his views on this subject. I now, therefore, regard the process of mountain formation to be briefly as follows: lines of thick sediments, rise of geo-isotherms and aqueo-igneous softening determine lines of yielding; then crushing together *horizontally* and swelling up *vertically* forms the chain; but once the yielding commences, then *mechanical energy is changed into heat*, which may thus be increased to any amount and produce true igneous fusion.

ART. XLIX.—*Notes of Observations on Jupiter and its Satellites;* by Prof. M. MITCHELL, of Vassar College. No. 2.

THE following observations were made with the equatorial telescope of the observatory of Vassar College, the object-glass of which is 12⅛ inches in diameter; the powers used varied from 200 to 250. Longitude of obs. $4^h\ 55^m\ 37^s$; lat. 41° 40′ 50″.

1872, Jan. 5. At $11^h\ 12^m$ P. M. the 3d satellite and its shadow, and the 1st satellite and its shadow, were seen on the disc of Jupiter at the same time. The 3d satellite was not round, but elongated in the direction of the belts of Jupiter, as small as the shadow of the 1st and nearly as dark, the color being brown. The 1st satellite was seen as a brilliant white spot, thrown upon the dark equatorial belt of Jupiter, yet it could be seen for only 25 minutes. The disappearance of the first satellite in transit does not seem to be a consequence of difference of brilliancy of color between it and the planet.

1872, Jan. 17. $8^h\ 15^m$ to $9^h\ 12^m$. A circular white spot was seen on the lower part of the broad belt of Jupiter, sufficiently defined to be measured.

On the 25th, at 8 P. M., a spot, apparently exactly like that seen on the 17th, was observed in the upper part of the broad belt, measuring nearly the same in diameter.

1872, Jan. 30. The 1st satellite which was known to be upon the planet could not be seen until it touched the limit at its egress, although its path must have lain wholly within the dark belt. It was by measurement smaller in diameter than its shadow.

1872, Feb. 2. The 2d satellite was seen as an irregular white spot when a little past the center of the disc. The shadow appeared to be larger than the satellite, but, on measuring, was found to be smaller.

1872, Feb. 7. The 3d satellite showed a very well defined disc with no spot. Its diameter was 2″·09.

1872, Feb. 26. The 4th satellite was occulted. It became very indistinct as it approached Jupiter. Its light was whiter than that of the whitest portion of the planet. It became invisible at $9^h\ 48^m\ 42^s{\cdot}64$.

1872, Feb. 28. The night was remarkably good. Two large white spots were seen on the equatorial belt at 7 P. M. They were well defined and were measured. They were visible for a short time only, and could not be seen to follow with the planet as it turned. Dark spots seemed to succeed them in the same position on the disc.

The 3d satellite was first seen to emerge from shadow at $7^h\ 5^m\ 43^s{\cdot}4$. It was fully out at $7^h\ 11^m\ 4^s{\cdot}4$. The occultation of the 1st satellite occurred at $9^h\ 6^m\ 39^s{\cdot}4$.

1872, March 1. The 1st satellite was seen to come out of the shadow at $6^h\ 49^m\ 9^s{\cdot}9$.

1872, March 7. The 1st satellite seemed to touch the limb, in transit at $7^h\ 13^m\ 51^s{\cdot}6$, was wholly on the disc at $7^h\ 19^m\ 0^s{\cdot}6$; after which it was seen for only ten minutes.

1872, March 16. Three dark spots were seen upon the principal belt, larger and as dark as the shadow of the 1st satellite, which was also on the planet's surface. The shadow seemed to become smaller and more distinct as it approached the limb; it was last seen at $7^h\ 56^m\ 36^s{\cdot}88$. By measurement the diameter of the shadow of the satellite was larger than that of the satellite. The 3d satellite was free from spots.

1873, Jan. 19. Observations on Jupiter began at $8^h\ 30^m$ P. M. The 3d satellite was known to be in transit, but could not be seen until it had passed the center of its path. It was then an irregular dark spot. It became more round and well defined and again indistinct, although there were no perceptible changes of light and shade on the disc of the planet, and the air was steadily improving.

1873, Feb. 4. At a little after 9 P. M. the 4th satellite was seen on the disc of Jupiter as a brownish-gray marking, not far from the preceding limb. It was lost for a time, but reappeared when near the limb. Like the other satellites in that position, it showed a disc similar to that of the moon seen through mist. It was first seen to protrude beyond limb at $9^h\ 35^m\ 45^s{\cdot}20$; was wholly off at $9^h\ 38^m\ 31^s{\cdot}20$. The 3d satellite, shining far from Jupiter, showed a disc irregular in shape and hazy in outline. The broad belt of Jupiter was slightly reddish.

1873, Feb. 17. Observations began at $7^h\ 32^m$. The shadow of the 1st satellite could be seen, thrown upon the planet, it was not round, but elongated in a direction perpendicular to the broad belt, upon which it was seen. As usually happens, the satellite was seen round and snowy white a few minutes before it left the disc; it was much whiter than Jupiter. 1st satellite was wholly off at $8^h\ 47^m\ 42^s{\cdot}5$; shadow last seen $8^h\ 52^m\ 00^s$.

1873, Feb. 25. The 2d satellite was occulted at $9^h\ 49^m\ 52^s{\cdot}6$.

1873, March 11. Jupiter was seen between flying clouds, but the seeing was excellent. A faint rosy tinge could be seen on the upper part of the broad equatorial belt, on which there was a large white spot. The 3d and 4th satellites showed distinct discs; that of the 3d was ruddy in color. The 1st satellite was occulted. It touched the limb at $8^h\ 46^m\ 23^s{\cdot}1$; was bisected by the limb at $8^h\ 48^m\ 17^s{\cdot}1$; was last seen at $8^h\ 51^m\ 23^s{\cdot}6$.

1873, March 13. Seeing excellent. Four lines in the broad belt were strongly marked, but no rosy tinge could be per-

ceived. The 2d satellite touched the limb at $8^h\ 30^m\ 12^s{\cdot}75$, was wholly within the limb at $8^h\ 38^m\ 00^s{\cdot}25$, after which it could be seen for a very few minutes.

1873, March 17. 10 P. M. The 3d satellite was seen as a dull irregular shading upon the disc of the planet. It became a well defined brownish-gray spot as it neared the center, seeming to be preceded by a minute grayish spot, possibly denoting an irregularity in the shape of the satellite. Diameter of 3d satellite $= 1''{\cdot}8$ as measured while in transit.

1873, March 28. The equatorial belt was marked by two large white spots, which seemed to narrow and elongate in an equatorial direction as the planet turned. The shadow of the 1st satellite passed from the disc at $7^h\ 22^m\ 44^s{\cdot}5$.

ART. L.—*Some remarks on the Geological Structure of a district of country lying to the north of the Grand Cañon of the Colorado;* by J. W. POWELL.

THE Colorado River of the West is formed by the junction of the Grand and Green; from this point the course of the river is a little west of south until the mouth of the Little Colorado is reached, and from this last mentioned point, its general course is to the west to the mouth of the Rio Virgen, where it turns again to the south.

The Grand Cañon extends from the mouth of the Little Colorado to the foot of the Grand Wash, a narrow, abrupt, cañon valley extending back from the Colorado River from a point about 60 miles above the mouth of the Rio Virgen. This profound gorge is more than 200 miles in length, and varies from 4,000 to nearly 6,000 feet in depth.

I propose, in this article, to discuss briefly some of the principal topographical and geological features of a district of country lying to the north of the Grand Cañon and south of the sources of the Sevier, east of the Colorado River, and west of the Grand Wash and Pine Valley Mountains.

The principal tributaries of the Colorado from the region under discussion, commencing on the west, are the Rio Virgen, the Kanab, Tapete River and the Paria. All of these rivers, for the greater part of their courses, run in deep gorges, and this is true also of their tributaries; so that the region is traversed by a labyrinth of profound cañons.

From a line some distance south of the Grand Cañon, to a line somewhat north of this region, all the geological formations have a general dip to the north. To the south, the upper formations have been eroded away, and in going from the bot-

tom of the Grand Cañon north to the plateaus in which the streams mentioned have their sources, we pass over the upturned edges of nearly 25,000 feet of geological formations. Commencing below, in the most southern bends of the Grand Cañon, we find about 1,000 feet of metamorphic crystalline schists, with dykes and beds of granite. In the lateral cañons, which enter from the north, we discover another group of rocks, composed chiefly of sandstones and shales, with varying local dips. Dykes of eruptive rocks penetrate these beds in many places, or pass through them in others, and large accumulations of igneous material are found. This group is non-conformable with the lower metamorphic rocks, for wherever the junction has been seen there are abundant evidences of extensive erosion intervening. The rocks are of Pre-carboniferous age. No fossils have been found in them, but the Carboniferous rocks lie on their upturned edges, so that there was a long period of erosion separating these formations also. The Carboniferous sandstones, limestones, and shales, next succeeding, are from 4,000 to 5,000 feet in thickness; then we have about 2,500 feet of what are deemed to be Triassic rocks; next we have 1,000 or 1,200 feet of Jurassic rocks; still surmounting these, we have 1,800 or 2,000 feet of Cretaceous beds, and then we reach Tertiary rocks, 3,000 or 4,000 feet in thickness in this district, but farther to the north obtaining a thickness of nearly 7,000 feet.

The most remarkable features of the country are the deep narrow cañons by which it is interrupted, making its exploration a task of no little magnitude. The cañon walls are towering escarpments of rock, that in many places cannot be scaled.

The class of features next in importance consists of long lines of cliffs which stand athwart the country. These cliffs are bold escarpments of rock, often many hundreds of feet in height, and scores or hundreds of miles in length; in many places quite vertical above, with a sloping *talus* at the foot. There are two series of these cliffs in the region under discussion, having their origin in very different causes. The cliffs belonging to one of the series have an easterly and westerly trend; the other a northerly and southerly.

The lines of cliffs which extend across the country in an easterly and westerly direction will be discussed first. Starting at the brink of the Grand Cañon and going to the north, we travel on the summit of Carboniferous formations, until we reach a line of cliffs from 100 to 400 feet in altitude. This escarpment is capped by a firmly cemented conglomerate containing many fragments of silicified wood, and over its surface are scattered many like fragments, and sometimes huge tree-trunks, which are the remnants of rocks at one time overlying

the conglomerate, but now carried away by erosion. Underlying this cap are variegated sandstones and marls. The whole group is probably of lower Triassic age. The silicified woods so abundant here are called by the Indians who inhabit the country, Shin-ar-ump; or, The arrows of Shin-aú-av. (Shinauav is the Hercules of their mythology.) To the cliffs they give the name Shin-ar-ump Mu-Kwan-i-Kunt, and we have adopted as the English name, Shin-ar-ump (or Arrow) cliffs. Still passing to the north a few miles, we reach the foot of a second line of cliffs, composed of red sandstone, and beds of lighter color, which are stained red on the surface. To this line the Indians have given the name Un-Kar Mu-Kwan-i-Kunt; we have adopted the translation, *Vermilion Cliffs*. This escarpment is often more than 1,000 feet in altitude, bold in outline and brilliant in color, and forms a conspicuous feature on the landscape. Still going to the north for a half day's journey or more, we reach the foot of another line of cliffs, which are capped by nearly two hundred feet of limestone, beneath which is found a bed of massive light-grey sandstone. The Indian name for this line is, At-sí-gar Mu-Kwan-i-Kunt, or, Gray Cliffs, which English equivalent we have adopted. The limestone at the summit contains Jurassic fossils.

Continuing to the north, Cretaceous beds are seen in the hills on either side; farther to the east these beds are found in two lines of cliffs, of which no farther mention will here be made. Having passed the Cretaceous rocks, and many hundreds of feet of lower Tertiary, we reach the foot of another line of cliffs composed of limestones and sandstones; some of these beds are of light pink color, and the exposed edges of all are stained in such a manner as to give the same bright color to the entire face of the escarpment. To this line we have given the name Pink Cliffs, being the English translation of the Indian name, Un-tsaw-ar Mu-Kwan-i-Kunt.

The general dip of all these formations, it will be remembered, is to the north, and in passing from one line of cliffs to another, where the same stratum is followed, a gradual descent is made; but the traveler in going north is almost imperceptibly passing on to later formed beds, until he meets with a line of cliffs where the ascent is made abruptly. The Shinarump Cliffs have been traced from a point west of the Rio Virgen to a point many miles east of the Colorado River,—a distance of nearly 200 miles; the Vermilion Cliffs have been traced somewhat farther, as have the Gray Cliffs. The Pink Cliffs are about 50 miles in length, and form the southern boundary of two extensive plateaus that are separated by Long Valley and the Valley of the Sevier, and have a general altitude above the sea of eight to nine thousand feet.

It will be seen from this description that to go from the bottom of the Grand Cañon to the summit of these plateaus, you must climb by a great geographical stairway, the steps of which have an attitude of many hundreds of feet, and a width of many miles. As the rocks dip to the north, the difference in altitude between the two points is only about 7,000 feet; but were the beds horizontal, the plateau would be more than twice that height above the river.

It is a remarkable fact that the geological formations do not terminate by a thinning out of the strata, or by gently eroded slopes, but most of them end abruptly in lines of cliffs; and where these are found it is invariably seen that the summits of the escarpments are composed of rocks of a homogeneous and firm texture, with underlying rocks that yield more readily to atmospheric degradation, that is, the rocks below are of heterogeneous composition, and are not friable.

Another of the important conditions under which these wonderful cliffs have been formed, is this: that the progress of the emergence of the folds above the general surface of the country, or level of the sea, was, in its earlier stages at least, little or no greater than the progress of erosion, so that the rocks were carried away quite as fast as they were exposed to denudation. It would be beyond the reasonable limits of such an article as the present to discuss this subject.

The lines of cliffs which have a northerly and southerly trend are due to abrupt displacements of the strata, either by faulting or folding. I propose to call these displacements broken folds, for reasons which will subsequently appear.

On the east side of the Grand Wash we discover a great fault extending across the Colorado River. The drop of the fault is on the west side of the fracture. The wall remaining *in situ*—for I supposed it to be a drop, not an uplift—stands in a bold escarpment which forms the eastern wall of the Grand Wash. To the escarpment we have given the name, Grand Wash Cliffs, and to the displacement, Grand Wash Fault.

Farther to the east another fault is discovered, approximately parallel to the first; the drop of the bed is also to the west. It extends from an unknown point south of the Colorado, in which direction it has been traced about thirty miles without discovering its terminus, to a point north of Tokerville. Its northern terminus has not yet been discovered. The displacement is from 1,300 to 2,800 feet. To the south it is a fault, but farther to the north it is seen to change gradually to a monoclinal fold. The broken edges of the rocks on the eastern side of the fault, which have not been displaced, stand in a remarkably steep escarpment, in much of its course a sheer precipice, impossible to be scaled even by men accustomed to

mountain climbing. Several small towns have been located along its foot, and the people have given to the cliffs lying to the south of the Rio Virgen, the name Hurricane Lodge, but in order to conform this to my general nomenclature, I have called it Hurricane Cliff. The line of cliffs north of the Rio Virgen we designate as Toker Cliffs; the displacement we call Hurricane Fault.

It will be observed that the direction of these faults is, in a general way, at right angles to the grand strike of the formations, and as the drop is to the west of the fracture, the local dip is easterly.

Going yet farther to the east about twenty miles, another fracture is discovered. This has been seen to extend south of the cañon twenty-five or thirty miles; how much farther it may continue is not known. It has been traced to the north through the Vermilion Cliffs, where Short Creek Cañon marks its position. Where it crosses the Shinarump Cliffs, the displacement is seen to be about 120 feet. On the north side of the Grand Cañon it is marked by a cañon valley about thirty miles long, to which we have given the Indian name, To-ró-weap. At the foot of the valley, on the brink of the Grand Cañon, the displacement was found to be 820 feet, and it appears to be still greater on the south side. We have named this To-ró-weap Fault, and to the clifts have given the same name. Again to the east another fault is discovered. We are not yet certain whether this extends to the south of the Grand Cañon or not; the most southern point where it has been seen is about ten miles north of the cañon, from which point it has been traced past Pipe Spring to the foot of Long Valley, thence up Long Valley to its head, and from thence across the divide to the head of Sevier Valley, down the Sevier Valley to Salina, and still continuing in a northerly direction up the Sanpete Valley to its head opposite Mount Nebo. The drop of the rocks is still on the western side of the fissure, and varies from 100 to 3,000 feet. In some places this displacement also appears as a monoclinal uplift. We have named this the Fault of the Sevier. Along its course several lines of cliffs are seen which have been designated as follows: the line on the east side of Sanpete Valley, Sanpete Cliffs. From the foot of Sanpete Valley to a point about midway up the valley of the Sevier, ranges of eruptive mountains are seen to the east, the rocks constituting which have probably emerged from the fissure of this fault, and have, to some extent, obscured it. South of these eruptive ranges, the eastern wall of the valley of the Sevier is a well marked line of cliffs, which forms the western boundary of one of the plateaus heretofore mentioned. To this line we have given the name Sevier Cliffs. Another line

is seen on the eastern side of Long Valley, named Long Valley Cliffs. Another fault is seen at the cañon of the Kanab. It has been traced about thirty miles; the drop is from 100 to 200 feet, and still on the west, but being inconsiderable, no well marked line of cliffs has been formed; this we call Karab Fault.

Another fault to the east marks the western boundary of a great plateau; this displacement, either as a fault or monoclinal uplift, has been traced to the northern sources of the Dirty Devil River, and it probably extends much farther; to this we have given the name Western Kaibab Fault. The eastern edge of the same plateau marks the line of another fault. This has been traced about fifty miles south of the Grand Cañon, and probably extends still farther, as a continuation of the line would strike the San Francisco Mountains, a vast group of volcanic tables and cones, and it is conjectured that the eruptive matter issued from the fissures of this fault and its branches; to this we have given the name Eastern Kaibab Fault; and like the Western, it has been traced far to the north, and it is believed to extend to Price River Valley; but here the drop changes and is found on the eastern side of the line.

Still other folds and faults have been found to the east, but none of them have been traced, having been seen only at points along their lines, and hence no farther mention of them will here be made.

This great system of broken faults has been a subject of much careful study. Each one has been traced along its line through the whole extent of the country under discussion. From time to time the drop had been measured, and a great variety of accompanying phenomena observed. Some of these facts are of much interest. In many places the faults are seen to branch; in others they suddenly or gradually change into monoclinal uplifts; in still others the drop marks but a part of the displacement; the edge of the fallen rock having caught on the wall remaining *in situ*, is turned up, so that below it appears as a fold, and above as a fault. In other places the edge of the fallen rock is bent down, and in still other places the rocks are not separated by well defined fissures, nor are they folded, but irregular masses of broken rocks intervene.

It is interesting to observe the way in which these two systems of cliffs are related. The eastern and western faults are of very irregular outline; sharp salients are set out on to the plains below, and deep re-entering angles are seen, and streams that head to the north and flow to the south, have cut cañons through the cliffs, so that it is possible to ascend these great steps by passing up a cañon way, rather than by climbing escarpments. Wherever a fault crosses one of these lines, the

latter is broken, and where the drop of the fault is to the west, the line of cliffs on the western side of the fault is thrown to the south, to a distance which is in direct ratio to the extent of the drop.

The bearing of these facts, in the study of the conditions under which these cliffs were formed, is very interesting, but I may not stop to discuss it farther here.

The lines of cliffs which are formed by the north and south faults are of much more regular outline, and are more rarely crossed by cañons, yet, in a few places they are thus cut by channels of streams. In some places these streams, in crossing the fault, run from the upper to the lower beds. In other places they run from the lower to the upper beds.

The next group of topographical features in this country, consists of the plateaus, to some of which I have heretofore alluded. East of the Grand Wash Cliffs, and west of the long cañon valley at the foot of the Hurricane Cliffs, and north of Grand Cañon, and south of a short abrupt fold that can be seen to extend between the two faults a little south of Fort Pierce, there is a great table, its surface having an inclination to the northwest, determined by the grand dip of the rocks to the north, and the local dip to the east, which is due to the faulting. To this we have given the name Sheáv-wits Plateau.

Between the Eastern and Western Kaibab Faults there is an extensive plateau, extending from the Grand Cañon to the foot of the Vermilion Cliffs; the Indian name for this is Kaibàb, meaning, Mountain lying down, and pleased with its significance, we have adopted the name Kaibab Plateau.

A triangular table of Triassic sandstone is seen between the Colorado, the Paria, and House Rock Valley; for this we have adapted the Indian name, Un-kár Kaiv-áv-i. The Indian name for the plateau east of the head-waters of the Sevier is Pouns-á-gunt, meaning, the Home of the Beaver; this name we have also adopted. The plateau west of the river they called *Mar-ká-gunt*, Home of the flowering bushes; I hardly need add that we were pleased to adopt this name also.

Through the great fissures of these faults floods of lava have issued and spread over the country in broad sheets, or run down into and filled the valleys. The earlier sheets of this basalt, being of firmer texture than the sedimentary rocks, have preserved the districts of country over which they have spread from erosion, and thus table-mountains have been formed. In our earlier studies we supposed that some of these mountains were masses of igneous rocks. On farther examination it was found that they are composed of sedimentary beds with caps of basalt only. Since the formation of such beds of basalt, atmospheric agencies have carried away

many hundreds of feet of the adjacent softer beds, leaving those portions of the bed, protected by the harder material, as table-shaped mountains. As erosion advanced on the sides of these mountains, the more friable material below was carried away, and the edges of the sheet of basalt undermined, and huge blocks have tumbled down, and, to a greater or lesser extent, obscured the true structure of the mountain. In many places later overflows have occurred in these mountains, not issuing from the summits of such tables, on the flanks, and it may be accepted as a general rule, that the later the flow, the lower the point from which it emerged.

In other regions these sheets of basalt seem to have been formed at a period later than those seen in the table mountains, so that only low mesas appear; but of course no definite line of demarkation can be drawn between such mesas and what we have designated as table mountains.

The expiring energies of these eruptive agencies have left great numbers of cinder cones standing in lines along the fissures. Many of these have well defined craters, and they everywhere form conspicuous features on the landscape.

Cañons and Valleys.

No sharp line of division can be drawn between cañons and valleys. For convenience, we designate intervening depressions caused by erosion, cañon valleys, but all of these excavated basins and troughs will be included under the general head of valleys.

This is a region almost everywhere of naked rock. The cañon walls, and cliffs, present vertical sections of strata of great magnitude, and the nakedness of the upper surface of the rocks, together with the exposure in the escarpments, make it possible to examine the geological structure of the country with great thoroughness; and conclusions may be reached with a degree of certainty elsewhere rarely attainable. Under these circumstances, it has been possible to understand the causes which have combined to determine the vast system of drainage, and to discover the relation in the direction of the valleys to the dip of the folds. I propose to classify the valleys of this country in the following manner:

Order first: transverse valleys, having a direction at right angles to the strike.

Order second: longitudinal valleys, having a direction the same as the strike.

Of the first order three varieties are noticed:

a, monoclinal, those which pass through a fold;
b, acclinal valleys, that run in the direction of the dip;
c, contraclinal valleys, that run against the dip of the beds.

Of the second order we also have three varieties:

A, anticlinal valleys, which follow anticlinal axes;

B, synclinal valleys, which follow synclinal axes;

C, monoclinal valleys, which run in the direction of the strike between the axes of the fold, one side of the valley formed of the summits of beds, the other side composed of the cut edges of the formation.

Many of the valleys are thus simple in their relation to the folds; but as we have two systems of displacements, a valley may belong to one class in relation to one fold, and to another in its relation to a second; such we designate as complex valleys. Again, a valley may belong to one class in one part of its course, and to another elsewhere in its course; such we designate compound valleys. It will farther be noticed that valleys may have many branches, but in relegating a valley to its class we consider only the stem of the valley proper, and not its branches.

A great diversity in the features of all these valleys is observed. Most of these modifications are due to three principal causes: first, a greater or lesser inclination of the rocks; second, the texture of the beds, that is, their greater or lesser degree of heterogeneity; the third class of modifying influences is found in the eruptive beds.

In the country under discussion the opportunities for studying the effects and extent of erosion are very great. The amount of erosion exhibited in the Grand Cañon alone is so great as almost to stagger belief. If a hundred mountains, each as large as Mount Washington, were tumbled into this cañon, they would scarcely fill it; but even this great amount does not furnish a practicable unit with which to measure the denudation of the country. A vast labyrinth of deep gorges has been excavated, extending to every part of this region, and should we compute the amount of rock necessary to fill these to the general level of the country, we should still have but a meagre term of comparison for the sum of the material which has been carried away by rains and rivers.

On the flanks of the folds which are found everywhere in the valley of the Colorado, we see the edges of formations which once, doubtless, extended quite over the folds. That these formations were once continuous appears evident from the following considerations: first, they are not seen either to thin out, or thicken up, and bear no evidences of having been immediate shore formations; second, they terminate in abrupt escarpments; third, they may be traced on either side of the folds, and seem to have the same lithological and paleontological characteristics; and fourth, outliers of the formations may be discovered in many places, that have withstood the vicissi-

tudes of erosion. The most remarkable of these are such as have been protected by sheets of eruptive rocks. In the Minkaret Mountains we find a group of basaltic tables and cones standing far out on the Carboniferous rocks. Twenty-five or thirty miles to the north we are able to study a series of shales, marls, and sandstones, of Triassic age, exposed in the Shinarump Cliffs. The surface of the country intervening between the mountains and the cliffs is seen to be Upper Carboniferous; but here, in the mountains, from 1,200 to 1,500 feet of Triassic rocks can be studied, and it is found that all the details of bedding observed in the cliffs are repeated on the sides of these tables; they are thus proved to be outliers of beds that formerly extended over the intervening region.

The contemplation of this vast extent of erosion will not stagger us, when we reflect that the sedimentary beds are evidences of an amount of erosion co-extensive with the magnitude of these formations, and anterior to that which we are now studying; but it is interesting to study erosion by observing its immediate effects, rather than examining beds which only give evidences that such an amount of erosion actually occurred,—without being able to trace it farther.

The general history of the geological operations recorded in this desolate waste can be briefly summed in the following statement: The deposition of beds, of many thousands of feet in thickness, in which are enclosed the relics of life which existed when these rocks were formed, and which were buried in their accumulating sands; then the folding and faulting of the whole series, and, *pari passu* with this, the excavation of a wonderful system of gorges, and the carving of a vast net-work of valleys, leaving behind towering cliffs stretching across the country in every direction, and still, *pari passu*, with the folding, and faulting, and denudation, great floods of lava were poured out from the interior to fill valleys, and form mesas, and tables, and mountain cones. And in the present period we have an *ensemble* of topographical features embossed on the face of the country, wild, grand, and desolate.

ART. LI.—*Remarks on certain Errors in Mr. Jeffreys's Article on "The Mollusca of Europe compared with those of Eastern North America;"* by A. E. VERRILL.*

IN the October number of the Annals and Magazine of Natural History, Mr. Jeffreys published an article upon this interesting subject, in which many important errors occur, due, no doubt, to the fact that the distinguished author is much less

* From the Annals and Magazine of Nat. Hist., IV, vol. xi, p. 206.

familiar with American than with European shells. But as the dredgings in connexion with the investigations of our fisheries by the U. S. Fish Commission were under my superintendence during the two past seasons, and Mr. Jeffreys alludes to the fact (though rather indefinitely) that he, by invitation of Professor Baird, accompanied us on several dredging-excursions in 1871, it seems necessary that I should point out some of the more important of these errors, lest it be supposed by some that the same views are held by me.

It is not my intention to discuss at this time the numerical results presented by Mr. Jeffreys; but I would remind the readers of his article that the regions compared are in no respect similar or parallel, and that it is scarcely fair to compare the shells from the entire coast of Europe with those from about 200 miles of the coast of New England, where the marine climate is for the most part more arctic than that of the extreme north of Scotland—and, moreover, that the last edition of Gould's "Invertebrata of Massachusetts" contains only a part of the species added to our fauna since the first edition was published in 1841, and very little of the great mass of facts in regard to distribution, &c., which have been accumulated by American naturalists during the last thirty years. Consequently that work is far from being a good "standard of comparison." To make a just comparison, all the shells on our coast, from Labrador to Florida, should be compared with those of Europe.

And without going into a long discussion of his peculiar views on the geographical distribution of our shells, I would remark that, to an American, it seems rather singular that most European writers, whether zoölogists or botanists, find it necessary to trace back to a European origin all the existing species of this country, and to suppose that they have "migrated" from Europe to America and other countries in spite of opposing currents and all other obstacles. Thus Mr. Jeffreys can imagine that our land and freshwater shells could have migrated from Europe all the way across Asia, the Pacific Ocean and North America in order to reach Canada and New England; but he does not seem to think it possible that they may have *originated* in America, and thence crossed to Europe in the direction of the prevailing currents and winds. Nevertheless geology teaches us that America was a great continent, in very early ages, when Europe was only a group of islands; that no other country is richer in the remains of terrestrial animals and plants connecting the Tertiary and Cretaceous ages with the present; that many of these supposed European forms (whether terrestrial or marine) can be traced back into our Tertiary formations quite as far (if not farther) than they can in Europe; and

that many of the genera of animals, and especially of plants, now found living in both countries, can be traced back to the Cretaceous in America and only to the Tertiary in Europe. Moreover the great number and diversity of the land and fresh-water shells of America (*e. g.*, of *Unionidæ*, *Melaniæ*, &c.), and the peculiar facts in their geographical distribution, cannot but convince any one familiar with the subject that they have *originated* in America at a very remote period; which is confirmed by the fact that many of these can be traced far back into our Tertiary formations. Nor are there sufficent reasons for supposing that those of our species living also in Europe have had a history different from those that are still peculiar to America.

Of course, no one will deny that certain species of land-shells have been introduced from Europe in modern times by human agency; but, so far as most of the identical species are concerned, it seems to us far more probable that America gave them to Europe, rather than the contrary, and this whether animals or plants, terrestrial or marine.

But the special errors to which I wish to call attention occur in the table of species, showing their geographical distribution. These relate both to the names and specific identity of certain shells, and to their geographical distribution. Although not agreeing with the author in regard to many of his remarks concerning the generic relations and names of species, I do not propose to discuss them here; for there seems to be no danger of their general adoption, either in Europe or America.

The following marine species (named as in Gould), which Mr. Jeffreys puts down as belonging to the region north of Cape Cod, actually belong properly to the region south of Cape Cod, extending in most cases to the Carolina coasts or beyond, while north of Cape Cod they are rare or local, viz :—*Cochlodesma Leanum*, *Mactra lateralis*, *Petricola pholadiformis*, *P. dactylus*, *Gouldia mactracea*, *Cytherea convexa*, *Venus mercenaria*, *V. notata*, *Gemma gemma*, *Liocardium Mortoni*, *Arca transversa*, *Modiola plicatula*, *Pecten irradians*, *Ostrea Virginiana*, *Anomia electrica* (not of Linn.), *Diaphana debilis*, *Cylichna oryza*, *Placobranchus catulus*, *Crepidula fornicata*, *C. plana*, *C. convexa*, *C. glauca*, *Ianthina fragilis*, *Bittium Greenii*, *Odostomia bisuturalis*, *O. seminuda*, *Turbonilla interrupta*, *Pleurotoma bicarinata*, *P. plicata*, *Nassa obsoleta*, *Buccinum cinereum*, *Diacria trispinosa*, *Loligo Pealii*.

The following, to which a northern distribution is likewise given, are also found far south of Cape Cod, and many of them belong quite as much to the southern as to the northern division; and some of them are decidedly southern, extending even to the Gulf of Mexico :—*Teredo navalis*, *T. megotara*, *T. chlorotica*, *Solen ensis*, *Machœra costata*, *Pandora trilineata*, *Lyonsia hyalina*, *Mac-*

tra solidissima, *Kellia planulata*, *Macoma fusca*, *Tellina tenera*, *Astarte castanea*, *A. quadrans*, *A. sulcata*, *Nucula proxima*, *Yoldia limatula*, *Mytilus edulis*, *Elysia chlorotica*, *Crucibulum striatum*, *Littorina rudis*, *L. tenebrosa*, *L. palliata*, *Lunatia heros*, *L. triseriata*, *Nassa trivittata*, *Melampus bidentatus*, *Alexia myosotis*.

Many others, not named in the above lists, are not limited by Cape Cod; but as they belong properly to the northern division, they are here omitted.

As an offset of these numerous instances in which he has unduly exaggerated our northern fauna, we find not one undoubted instance of an error on the other side, among the marine shells.

The distribution indicated for our land and freshwater shells is even more erroneous. It is sufficiently evident that Cape Cod is in no sense a proper boundary between the northern and southern fluviatile and terrestrial species; but, disregarding this, there are no reasons whatever for most of the special indications that he gives.

Thus he gives the northern distribution to all of the sixteen species of *Sphærium* and *Pisidium*; but most of them are well known to be widely distributed over the eastern, middle, and western parts of the United States, some even extending to the southern parts. *Unio complanatus*, *U. nasutus*, *Margaritana arcuata*, and *Anodon implicatus* are indicated as distributed north of Cape Cod; but all these are found over most of the northern and middle states and some in the western, while the last one is somewhat rare at the north. But *Unio radiatus*, *U. cariosus*, *U. ochraceus*, *Margaritana undulata*, *M. marginata*, *Anodon fluviatilis*, and *A. undulatus* are put down as southern. It would certainly be difficult to show that these, as a group, are more southern than the previous lot; for most of them have nearly the same wide distribution, and all of them, except *U. cariosus*, occur even in Maine. Some of them (as *U. radiatus*, *M. undulata*, and *A. fluviatilis*) are the most abundant species in all the waters of northern New England and New Brunswick. The distribution given for the species of *Valvata*, *Melantho*, and *Amnicola* is equally faulty.

All of the eighty-one species of *Helix*, *Hyalina*, *Macrocyclis*, *Limax*, *Pupa*, *Vertigo*, *Succinea*, *Arion*, *Zonites*, *Tebennophorus*, *Limnæa*, *Physa*, *Bulinus*, *Planorbis*, and *Ancylus* are set down as having the northern distribution, except *Hyalina Binneyana*, *Pupa fallax*, *Limnæa catascopium*, and *Physa ancillaria*. But every American conchologist knows that nearly all of those species are very widely distributed over North America, east, west, north, and south, many of them being limited only by the Gulf of Mexico on the south and California or the Pacific on the west. Nor is there any reason for the distinction made in the

case of the four species named above; for these, though differing among themselves, have the same distribution as many of those put down as northern, while *H. Binneyana* and *P. ancillaria* certainly have a very northern range, for they are abundant in Maine, New Brunswick, and Canada.

It is evident that such numerous errors of this kind render the paper, so far as geographical distribution is concerned, quite worthless; for it is sure to mislead.

Most of these errors might have been easily avoided had the author depended less on Gould's work and more on the recent works of American conchologists; for there is no lack of data in regard to the distribution of most of our shells. Even Dr. Stimpson's "Shells of New England" (1851), if consulted, might have saved most of the errors in regard to the distribution of the marine shells.

The fact that there is in the southern and shallower parts of the Gulf of St. Lawrence an isolated colony of southern shells, may have misled Mr. Jeffreys in many cases, especially as he evidently consulted the Canadian collections much more than those of the United States, many of the largest of which he did not see at all. In respect of erroneous identifications and the reduction of certain species to varieties, there is also much to be said; but this article is already so long that it will be necessary to refer only to some of the more obvious and important errors of this kind, leaving the rest to be discussed more fully elsewhere.

Every naturalist should be willing to allow his fellow naturalists full liberty of opinion with respect to the specific identity or difference of closely allied forms; and no one can claim to be infallible in such matters. Some of the errors to be mentioned do not, however, come under this head; for the species united have only remote affinities. Nevertheless the naturalist who has collected and carefully studied animals in their native haunts, under various circumstances, in many localities, and in great numbers, has, other things being equal, a very great advantage in these matters; and therefore I believe that Mr. Jeffreys would in most cases agree with me had he collected and studied as many American shells as I have, during the past fifteen years, or if he were as familiar with them as he is with the British species. In most of the cases to which I refer, my own conclusions are in harmony with those of Dr. Stimpson, who devoted so many years to collecting and carefully studying our shells, and who is well known for his accuracy in such matters. And it would be strange indeed if all American naturalists, as well as many eminent foreign ones, have always been making such ridiculous blunders in regard to some of our most familiar shells as Mr. Jeffreys would have us believe.

Thus he states (p. 240) that "*Gemma gemma*" (or *Tottenia gemma*) is the young of *Venus mercenaria!* But it has long been known to European as well as American conchologists that the animal of *gemma* is very different from that of *mercenaria*, and quite peculiar; that the hinge is constructed on a very different type is well known; and Prof G. H. Perkins has shown (Proc. Bost. Soc. N. H., 1869, p. 148) that *gemma* is *viviparous*, producing about three dozen young, with well-formed shells, at one time. Moreover, the young shells of *mercenaria*, smaller than the adult *gemma*, are sufficiently abundant on our shores, and may be seen in many American collections; they are certainly very unlike the *gemma* in form, sculpture, and hinge, as has been well known for more than thirty years.

Again, he states that *Arca transversa* is a variety of *Arca pexata*, the former being put down as northern, the latter as southern. That these shells are widely different in form and in the structure of the hinge is well known; for Dr. J. E. Gray many years ago established a new genus (*Argina*) for the latter, on account of its very peculiar hinge. That the animals are also quite different I can assert from personal observation. Moreover, the differences in the hinge, epidermis, and form are remarkably constant; and, finally, the two species have the same geographical range from Cape Cod to South Carolina, and are often found together. Both are very common in Long Island Sound and New Haven harbor; and I have examined hundreds of specimens of both species without finding the slightest evidence in favor of Mr. Jeffreys's views. Indeed, they are only distantly related, and evidently belong to distinct genera, *Argina* and *Scapharca*, where several writers have placed them.

He also states that *Mactra ovalis* is a variety of *M. solidissima*. He may not have seen a specimen of the true *ovalis*, for it is not common in collections; but the genuine *ovalis* is certainly a very well-marked species, widely different from the *solidissima*. They differ greatly in the hinge, epidermis, form of shell, and position of the umbos: moreover, the animals are also quite different. Both occur together of equal size in the Bay of Fundy; but the former is not known south of Cape Cod, while the *solidissima* is abundant everywhere along our sandy shores to South Carolina.

Concerning *Astarte castanea* he says, "Perhaps a variety of *A. borealis* Ch.;" but *castanea* is one of the best-defined species in this difficult genus, varies comparatively little, and does not extend far north, its range being decidedly *southern*. It is perfectly distinct from *A. borealis*. He reduces *A. quadrans* to a variety of *A. castanea*, and gives it a name that is quite uncalled for, even if this view were correct. He then makes *A. Port-*

landica a variety of *A. compressa;* but I have already shown (Amer. Journ. of Science, April, 1872) that it is a variety of *A. quadrans.* His arrangement of the other species of *Astarte* is equally objectionable, but it is not necessary to discuss them here.

The *Pecten fuscus* Linsley is given as the young of *P. irradians,* from which it is very distinct; but the writer has shown (Amer. Journ. of Science, vol. ii, p. 361, and vol. iii, p. 213, 1871–72) that it is really the young of *P. tenuicostatus.*

Dekay is given as the authority for *Æolis salmonacea* and *Æ. gymnota;* but they were both described by Couthouy in 1838, from whom Dekay borrowed both the descriptions and figures, five years later.

He states that *Dentalium dentale* (non Linn.) is a variety of *Entalis striolata,* and that the latter is a variety of *D. abyssorum* Sars; but both of these statements are incorrect. The first is the *Dentalium occidentale* Stimpson, and is a true *Dentalium,* entirely different, generically and specifically, from the *striolata;* and the latter is also quite distinct from *abyssorum.* Possibly Mr. Jeffreys has not seen perfect specimens of all the American species; otherwise, I cannot understand how he could have made these statements.

He is correct in considering *Crepidula glauca* a variety of *C. fornicata,* as others have done before him; but he has adopted a serious mistake, made by several other writers, in regarding *C. plana* (or *unguiformis*) also as a variety of *C. fornicata,* from which it is really very distinct. It is a very common error to suppose that this species always inhabits the inside of dead univalve shells; for it very often occurs on the *outside* of such shells, on stones, the back of *Limulus,* &c., and is frequently associated intimately with *fornicata* in all these situations; but nevertheless it always retains its essential characters, under all circumstances. The typical *fornicata* is also often found with it, plentifully, on the *inside* of dead shells.

Nor can *Margarita acuminata* be the young of *M. varicosa;* for in our collection there are full-grown specimens of both, equal in size, from Labrador.

There is no sufficient reason for adopting the name *Lacuna divaricata* in place of *L. vincta;* for it is not the *Trochus divaricatus* of Linné (1767), although it is the shell described under the same name by Fabricius in 1780, as shown long ago by Dr. Stimpson and others. Fabricius made a mistake which we have no right to perpetuate; nor does "usage," to which Mr. Jeffreys so often appeals, sanction the change.

The *Lunatia triseriata* is not, as Mr. Jeffreys thinks, the young of *L. heros,* but only a color-variety, as the writer had previously shown (April, 1872). Both varieties occur together,

from the smallest to the largest sizes; but the former sometimes becomes plain-colored before reaching maturity. There is no evidence that *Natica clausa* is the *Nerita affinis* of Gmelin, but quite the contrary; for the latter was placed in the section of *umbilicated* species, was described as *silvery within*, and came from New Zealand! It is probably one of the Trochidæ, and certainly could not have been this *imperforate Natica*.

In this place I shall not enter into a discussion of the numerous cases in which the author has reduced the American shells to "varieties" of the European species, because in many of these cases there must long be great diversity of opinion, and for most purposes it matters little whether these closely related forms be called "varieties" or "species," so long as the actual differences are recognized. But since Mr. Jeffreys has evidently made so many important mistakes in his article in regard to the identity of species, and has united those that have no near affinities, as already shown, it is logical to conclude that he may have made other mistakes in the case of more critical species. He must therefore pardon us if we regard his decisions in all these cases as at least doubtful, until confirmed by other evidence.

ART. LII.—*Note on the use of a diffraction "grating" as a substitute for the train of prisms in a Solar Spectroscope;* by Prof. C. A. YOUNG.

SINCE the diffraction spectrum differs from a prismatic spectrum of the same length in having the less refrangible rays more widely dispersed, it some time ago suggested itself that a so-called "*gitter-platte*," or "grating" of fine lines, might advantageously replace the prisms in spectroscopes designed for the observation of the solar prominences through the C line. In this idea I was strongly confirmed on seeing last winter some of the beautiful gratings ruled upon speculum metal by Mr. Chapman, Mr. Rutherfurd's mechanician. The spectra furnished by these plates far exceed in brilliance and definition anything of the kind ever before obtained.

Through the kindness of Mr. Rutherfurd I have recently come into possession of one of them, having a ruled surface of something more than a square inch, the lines being spaced at intervals of $\frac{1}{6400}$ of an inch. Combining this with the collimator and telescope of a common chemical spectroscope we get an instrument furnishing a spectrum of the first order in which the D lines are about twice as widely separated as by the flint glass prism of 60° belonging with the original instrument. In the

neighborhood of C the dispersion is nearly the same as would be given by four prisms.

The spectra of the higher orders are generally not so well seen on account of their overlapping each other, but fortunately with one particular adjustment of the angle between the collimator and telescope, the C line in the spectrum of the third order can be made to fall in the vacant space between the spectra of the second and fourth orders, and we thus obtain an available dispersion nearly the same as that of the instrument I am accustomed to use.

On applying the new instrument to the equatorial, I found (under atmospheric conditions by no means favorable, though the best that have presented themselves as yet), that in the first order spectrum I could easily see the bright chromosphere lines C, D_3, and F; I could also, though with great difficulty, make out $H\gamma$, (2796 K). On opening the slit the outline of the chromosphere and the forms of the prominences were well seen, both in the spectra of the first and third order, quite as well I think as with my ordinary instrument in the same state of the air. The spectra are of course fainter, but as this loss of light affects the back ground upon which the prominences are projected, as well as the objects themselves, it does not materially injure their appearence.

The grating is much lighter and easier to manage than a train of prisms, and if similar ruled plates can be furnished by the opticians at reasonable prices and of satisfactory quality, it would seem that for observations upon the chromosphere and prominences they might well supersede prisms.

Dartmouth College, May 9, 1873.

SCIENTIFIC INTELLIGENCE.

I. Geology.

1. *Note on the occurrence of the Trias in British Columbia;* J. D. Whitney. From a letter to one of the editors, dated Cambridge, Mass., May 7, 1873.—Several years ago, one of the officers of H. M. Surveying ship Hecate brought to the office of the California Survey a slab of rock, which had been collected somewhere up the coast far to the north of San Francisco, but the exact locality of which I was unable to ascertain, as the specimen was left in my absence. The slab was to me especially interesting as it closely resembled, both lithologically and paleontologically, our Plumas County Triassic slates.* Indeed the specimen looked so familiar that for some time I could hardly convince myself that there was not some deception about it.

* See Geol. of Cal., Vol. i, p. 309.

It was on the strength of this specimen that I ventured to extend the range of the Alpine Trias as far as British Columbia, in the little sketch I sent of our results, and which was published in this Journal for August 1864 (vol. xxxviii, p. 261). I did not fail to impress on Mr. Dall, when he started for Alaska, the importance of keeping a sharp look-out for the fossils of this interesting formation. He was not, however, so lucky as to fall in with any fossiliferous deposits of importance, nor was he able to throw any light on the occurrence of the specimen brought by the Hecate.

M. Pinart, a French explorer, however, has been more fortunate, since it appears from a communication made by M. Fischer to the French Academy of Sciences, last December (See Comptes Rendus, lxxv, 1784), that M. Pinart collected specimens from the entrance of Pavalouk Bay, which were densely crowded with a species of *Monotis*, and which M. Fischer refers to the Alpine Trias, thus in all probability, extending the range of this interesting formation, not merely as far as British Columbia, but even to the Alaskan peninsula. It is possible that the Hecate's specimen was from the very region visited by M. Pinart. This is, indeed, the more probable since, in spite of all my inquiries, I have never yet been able to learn of any fossiliferous rocks cropping out along the coast of British Columbia, or any where on the mainland north of our boundary.

M. Pinart also brought from the Alaskan peninsula (locality Aniakchak and another bay near Mount Chiginagak) specimens of rock containing *Aucella*, in all probability of Jurassic age, this formation having been previously recognized in that region by Grewingk. As the *Aucella* is the most abundant and characteristic fossil of the Jurassic slates of the gold-region of California, this occurrence is also not without interest to us. Judging from the discoveries of Messrs. Grewingk and Pinart, there is a good field for paleontological investigation, as well as for the study of volcanic phenomena, on the Alaskan peninsula and among adjacent groups of islands.

It is interesting to notice how this remarkable grouping of fossils which characterizes the Alpine Trias, and which seemed for a time to have such a limited range, has now been traced all around the world, New Zealand, New Caledonia, the Pacific coast of North America, High India, Spitzbergen; these are localities in which the peculiar *Monotis*- and *Halobia*-bearing slates have been found within the past ten or fifteen years.

2. *Notes to page* 438, *on mountain-making;* by J. D. Dana.—(1.) Although no case of unconformability between the Carboniferous and the underlying Paleozoic is yet distinctly made out in the Sierra Nevada, the Great Basin, or the Wahsatch, such occur farther south according to Mr. J. W. Powell, in the vicinity of the Grand Cañon of the Colorado. (See page 457 of this volume.) The fact that Whitney has found no rocks lower than Carboniferous in the Sierra may be a consequence of the same uncon-

formability beneath these mountains. But in the region of the Cañon, the Carboniferous, Triassic, Jurassic, Cretaceous and Tertiary beds are all conformable. The epochs of mountain-making over the Pacific slope south of the latitudes of the Wahsatch Range, and also of that of the north, were different from those within these latitudes.

(2.) Mr. James T. Gardner, in a letter of May 8th, informs me that in his opinion all the more important mountain ranges of the Great Basin (in Nevada) are included in a chain trending about N. 40° E., the whole having a breadth across the trend of 120 miles. Austin, in Nevada, lies near the center line of the chain. To the west of this elevated region is the great depression where the rivers of Nevada evaporate in Carson, Humboldt, Pyramid, Mud, and other lakes; and to the east is the great depressed area of Salt Lake. On this view, if these mountains were made at the close of the Jurassic, there were formed at this epoch three lofty synclinoria, the Sierra, the Humboldt and the Wahsatch, or the western-border, central and eastern-border chains of the Great Basin. The precise determination of the epoch of origin of the Humboldt chain is therefore of much importance.

3. *Geological Survey of the Territories in charge of Dr. Hayden, under the Department of the Interior. Organization and plans for the year* 1873.—The Department of the Interior U. S. Geological Survey of the Territories, in the charge of Prof. F. V. Hayden, was reorganized last winter and placed in condition to carry on systematic and connected surveys with a permanent corps of assistants. Congress changed the name to Department of the Interior U. S. Geological and Geographical Survey of the Territories, as it has been found desirable to have topographical work conducted under the influence of geologists. Variations of the surface forms are so indicative of structural changes in the western mountains, that the geologist uses topography as one of the principal branches of his science. The experience of the 40th Parallel Survey has been that the opportunities for mutual criticism and discussion between the gentlemen representing the two departments of geology and topography have been of the greatest advantage to both.

The action of Congress in uniting these two branches of natural science under one organization promises the best results. The scientific corps of the Survey now consists of Prof. F. V. Hayden, Geologist-in-charge, and his staff of three assistant geologists, several distinguished paleontologists, a botanist, a zoölogist, a photographer and an artist; a geographer and his staff of three topographers; three assistant topographers, a meteorologist, and a draughtsman; a quartermaster and his assistant.

The field of operations authorized by Congress for the coming season is the Territory of Colorado, and that part of Utah lying east of the Green River. It is bounded on the north by the belt of the 40th Parallel Survey, and the primary triangulation will be a part of the same system carried across from the Sierra Nevada by that survey. The work will be based upon a trigonometric

survey connected with measured bases. Several of the principal geodetic stations will be determined astronomically by the U. S. Coast Survey, whose experience and training as field astronomers renders their work above all question.

The area to be examined is divided into three districts; the detailed survey of each being entrusted to separate parties, consisting of an assistant geologist, a topographer and assistant, and a naturalist. The geologist is responsible for the report on the geology of his district, and has full credit for it as independent work. The topographer carries the secondary triangulation, and with his assistant works out the topography.

The chief geologist, with an independent party, makes such general and special examinations of the country as are necessary for so great a scientific work. The geographer carries on the primary triangulation and superintends the business and work of the field parties.

The head quarters of the Survey will be at Denver, Col. Ter., from which point the field parties will be supplied by the quartermaster.

First operations will be commenced about the middle of May, and it is hoped that the Survey will soon be able to give to the country accurate maps and descriptions of that most interesting region of the Parks of Colorado, and their encircling groups of snowy summits.

4. *Volcanoes of Hawaii.* Copy of a letter from Rev. TITUS COAN to Prof. LYMAN, dated Hilo, Hawaii, Feb. 14th, 1873.—"You have seen an account of the eruption within the great summit crater of Mauna Loa in August, 1872. This continued for two or three weeks. On the 27th of January of the present year, we had the grandest display from the crater that I have ever seen. The action within it was vehement, and the scene marvelously brilliant. The great mural pit was in awful ebullition, and so violent was the raging of the molten sea within, that herdsmen of Reed and Richardson's ranch, on the eastern slope, reported the mountain as constantly quivering like a boiling pot. At Kapapala in Kau, at the base of the mountain, both foreigners and natives assert that they distinctly heard the swash of the fiery liquid, like the roaring and surging of a rushing river. The sheen of light which rose thousands of feet heavenward, and spread like a burning firmament over the mountain, was truly magnificent. At times the splendor was so vivid and so extended that observers called out the whole neighborhood to witness the scene; some thought they saw the fiery river rushing down the side of the mountain; and numbers were sure that it was half way down the side, and that it was coming toward Hilo in hot haste. This, however, proved an optical delusion. The molten sea was confined within the deep crater, but it was fearfully grand. Parties were planning a visit to the scene of action, when suddenly the great furnace ceased blast. This was a little tantalizing, but as we had all been favored with free tickets to a royal display of fireworks, we could not mourn.

Kilauea has been very active for months, and vast changes have been made in the great pit. The overflowings within it have been frequent and abundant; hills of lava have been heaped up in the southern part of the crater, and the deep central basin is fast filling."

5. *Geology of Ohio.*—The first part of the Final Report on the Geology of Ohio, under the charge of Prof. J. S. Newberry, is just now leaving the press. It constitutes the first half of the first volume, and treats of the Geology of the State, and will extend, as we learn from Dr. Newberry, to 680 pages, and contain 25 maps and sections. Part II. of the same volume treats of the Paleontology, and will make about 450 pages, and be illustrated by 50 plates. This second part is promised by July 1st.

The sheets of nearly the whole of Part I, and some of the finished plates of the Paleontology, are now before us, and they show that Dr. Newberry, and his associates in the work, have placed the State under great obligations to them by their labors. Among the important questions in American geological history apparently settled by the survey is the fact that the "Cincinnati uplift," raising the region from Lake Erie southwestward into Tennessee, took place at the close of the Lower Silurian. We defer a further notice until a complete copy is received.

6. *The Upper Coal Measures west of the Alleghany Mountains;* by J. J. Stevenson, Prof. Geol. Univ. City of New York. 30 pp. 8vo. (Am. Lyc. Nat. Hist. N. Y., x, 226; also Salem, Mass.)—The author of this memoir shows, from a careful study of the region of western Pennsylvania and Ohio, and its various coal beds and accompanying strata, that the Cincinnati axis had its highest elevation before the deposition of the Upper Coal-measures began; that therefore the Upper Coal-measures of this region and of Indiana and Illinois were never united, and probably not the Lower Coal-measure basins. The Upper Coal-measures originally extended as far west as the Muskingum River in Ohio. The paper contains many sections illustrating the relations of the Pittsburg and higher coal beds.

7. *Report of Progress of the Geological Survey of Canada for* 1871–72; by Alfred R. Selwyn, F.G.S., Director. 154 pp. 8vo. Montreal, 1872. (Dawson Bros.)—This volume contains a valuable report by Mr. Selwyn on a journey to British Columbia, and an examination of the geology of the region, including the coal beds of Vancouver Island; also a special Report on the Vancouver Coal fields, by Mr. Richardson; on the Plants, by Dr. Dawson; and on the Coal, by Dr. T. Sterry Hunt. There are other reports on the geology of Canada, by Mr. Bell, Mr. McOuat, Mr. Vennor; of New Brunswick, by Mr. Bailey; and mining and mineral statistics, by Mr. Robb. The genera of the plants collected from the Vancouver coal beds (at Naniano and North Saanich) are, according to Dawson, *Tæniopteris*, *Taxodium* (*T. cuneatum* Newb.), *Sequoia* (*S. Langsdorffii* Heer), *Sabal* (a fan-palm), *Palmacites* (fragments of a leaf), *Populus*, *Quercus*, *Platanus*, *Cin-*

namomum (*C. Heeri* Lsqx.), *Taxites*, *Cupressinoxylon*. Dr. Dawson states that the plants led Lesquereux and Heer to refer the beds to the Tertiary, they being nearly allied to the Miocene; but that Newberry has shown that the evidence of the associated marine fossils makes them Cretaceous, which is the opinion now generally accepted, the species including *Ammonites*, *Baculites*, etc.

8. *Transactions of the Edinburgh Geological Society*. Vol. II, Part I.—This number of the Transactions of the Geological Society of Edinburgh contains the proceedings of the Society from November, 1869, to April, 1872, and includes many excellent papers. They range through all departments of geological science, while chiefly occupied with the geology of different parts of Scotland. The number opens with the address of Archibald Geikie, Esq., Director of the Geological Survey of Scotland, and President of the Society, in which some important points in geology are ably discussed. He offers just criticisms on the uncertainties and inconsistencies in lithological science, even that of Germany, where the subject has received most attention. The fact that specimens of *dolerite*, *anamesite*, *basalt*, *amygdaloid*, or *amygdaloidal dolerite* and *tachylite* (obsidian-like) may all be collected from a single dike in Scotland, is mentioned as an example of the multiplying of names and divisions, without sufficient distinctions, and as evidence that the geological characters and relations of the rocks have not been properly considered by those who have drawn out the systems of classification. The evil from this source is great. Rocks cannot be treated and arranged as if chemical or even as mineral compounds, or on the basis of any physical characters, by mere laboratory work, without a loss of three-fourths of all that is of geological interest in their relations.

9. *Annual Report of the State Geologist of New Jersey for the year* 1872. 44 pp. 8vo. Trenton, N. J., 1872.—This Report of Prof. G. H. Cooke is occupied with valuable information respecting the ores and mines, and various economical mineral products of the slate. He mentions the opening of a mine of mica, a mile north of Broadway, in Warren Co., in a granite vein intersecting gneiss. Some of the plates of mica are more than a foot across.

10. *Das Elbthalgebirge in Sachsen*, von Dr. H. B. Geinitz.—The second number of the second part of Dr. Geinitz's work appeared near the close of 1872. It contains descriptions and figures of the Brachiopoda and Pelecypoda of the Middle and Upper Quader. The figures occupy seven crowded plates.

11. *A Myriapod in the Permian*.—Dr. Geinitz has described and figured (Sitz. Nat. Ges. "Isis," 1872, pp. 125) a Myriapod from the Permian (Rothliegende or Dyas) of the vicinity of Chemnitz. He calls it *Palæojulus dyadicus*, a name that indicates its relations to Iulus.

12. *Tafeln zur Bestimmung der Mineralien*, von Franz v. Kobell. 108 pp. 12mo. München (J. Lindauer).—The *tenth* edition of von Kobell's well known and excellent tables for the determination of minerals has just been issued at Munich. It is an indispensable aid to the student in mineralogy.

13. *Supposed evidence of Man in the Miocene near the Dardanelles.*—J. LUBBOCK communicates to "Nature," of March 27th, the information that a letter from Mr. F. Calvert to Mr. E. Calvert announces the discovery in beds, regarded as Miocene Tertiary, of bones supposed to be of the Mastodon or Dinotherium, having on them etchings of figures of animals.

II. BOTANY.

1. *Nervation of the Coats of Ovules and Seeds.*—A brief article by Van Tieghem in Comptes Rendus, Aug. 14, 1871, and Ann. Sci. Nat., Nov., 1872, and a long one in the latter Journal by LeMonnier (apparently Van Tieghem's pupil), develop clearly the former's view respecting the morphological nature of the ovule. He deduces the foliar nature of its envelope from its "libero-vascular system," which is that of the leaf. It answers, as has been before explained, to a marginal lobe of a carpellary leaf transformed and convolute around the nucleus, which, being destitute of vascular tissue, is a "parenchymatous excrescence," a *trichome*, to use the recent term of the Germans. LeMonnier sums up the conclusions thus: 1. The ovule always consists of a lobe of a carpellary leaf, folded around a cellular *mamelon* inserted upon the medial line of the lobe: 2. in Angiosperms upon the upper or *trachean* face of the leaf; in Gymnosperms upon the lower or *liberian* face. 3. The embryo, although discontinuous from the tissues of the mother plant, has determinate relations of position: not only is the radicular extremity always directed to the micropyle, but its principal plane is generally perpendicular to or parallel with that of the seminal lobe. 4. The primine, characterized by the presence of vascular bundles, is commonly the only membrane which persists in the mature seed; the secundine, except in rare cases (*Euphorbiaceæ*), is only a deduplication of the primine, and is mostly transitory. A. G.

2. *Supposed American Origin of Rubus Idæus.*—Our cultivated Raspberry is an importation from Europe. Our native Red Raspberry, *R. strigosus*, however, is so near it that the specific distinctness has been in doubt; and specimens from British America and the Rocky Mountains certainly occur which a botanist must needs refer to *R. Idæus* itself. In his studies of the European *Rubi*, Prof. Areschoug (in Botaniska Notiser, 1872, and in a translation by himself in Trimen's Journal of Botany, April, 1873, p. 108, etc.) makes prominent and important the fact that *R. Idæus* has no near relative, or in other words, is the sole Raspberry, in Europe, but in mode of growth, in the bark, etc., as well as in the fruit, accords with American species,—with one of them so closely that all who have come to the conclusion that species have a history must needs infer a community of origin. Areschoug concludes, accordingly, that "this species did not originally have its home in Europe, but its origin is to be found in the east of Asia, viz: Japan and the adjacent countries, or perhaps in North America." It is one of the members of that old boreal flora (as we suppose) now mainly East Asiatic and North American, which

has found its way to, or held its place, in the north of Europe somewhat exceptionally. Both *R. strigosus* and *R. Idæus* inhabit Japan and Mandchuria, and Maximowicz regards them as forms of a common species. Prof. Areschoug adopts the now familiar idea "that the Asiatic and North American floras have reciprocally mixed with each other by passing Behring's Straits and the islands which in its neighborhood form a bridge between the two continents;"—which is a partial explanation of a problem that has to be treated far more generally now that we have reason to believe that this flora formerly filled the Arctic zone. He thinks, moreover, that the simple-leaved frutescent species (also extra-European) are the ancestors of those with divided leaves,—but this is a speculation of a different character, upon which little or no evidence can be brought to bear. A. G.

3. *Gelsemium has dimorphous flowers*, the stamens and the style reciprocally long and short. This was observed by Mr. Canby and myself this spring, but the long-stamened condition is the most common. It has already been noticed by Chapman, but it is worth calling attention to, as it was overlooked in Gray's Manual, as well as by A. DeCandolle and Bentham in their monograph of *Loganiaceæ*. The stipules are reduced to minute and glandular, deciduous points. A. G.

4. "*A New Textile Plant*, allied to the Nettles (*Laportea Canadensis*), has recently been imported from the Alleghany Mountains into Germany by M. Roezl. The plant is perennial and capable of enduring the climate of central Germany. Further experiments are needed ere the commercial value of the plant can be determined." *Gardener's Chronicle.*—Some in this country are old enough to remember a former trial. It was taken to England and Ireland fifty years ago by a Mr. Whitlow, with much ado, and was to take the place of flax. A. G.

5. *Hooker's Icones Plantarum.*—Part II, of Vol. II, new series, just issued, contains plates 1126 to 1150. The figures are chiefly of *Rubiaceæ* and *Compositæ*, and illustrate the new part of the *Genera Plantarum*. *Luina hypoleuca*, of Lyall's collection in Oregon (and which has lately been detected in California), is the only North American plant in this fasciculus. The name is evidently an anagram of *Inula*. The genus is probably too near *Tetradymia*, which sometimes has glabrous achenia. From the figures of the Rubiaceous genus *Heterophyllæa*, it may be rather confidently surmised that it has dimorphous flowers, in the manner of *Houstonia*, *Mitchella*, etc. A. G.

6. *Bentham and Hooker, Genera Plantarum.*—Part I. of Vol. II. of this most important work was published in April, and has come to hand. It contains, including an index, 554 pages: the whole, except about a dozen pages, devoted to the two great orders, *Rubiaceæ*, 337 genera, and *Compositæ*, of 766 genera, notwithstanding a very great reduction of old genera. A critical notice of this work must be deferred. The title page states that the Genera Plantarum is sold in London, by Lovell Reeve & Co.,

and by Williams & Norgate. It may be ordered from any principal bookseller. But any botanists who find it difficult to procure the work otherwise, may be supplied upon application to Harvard University Herbarium. A. G.

7. Wm. S. Sullivant.—This distinguished Bryologist and most admirable man died at his residence, Columbus, Ohio, on the 30th of April last, after an illness of about three months, at the age of 70 years. A biographical notice will be given in the ensuing number of this Journal. A. G.

III. Astronomy.

1. *Telescopic Observations of Meteors.*—Dr. Galle, of Breslau, has recently discussed the interesting question whether multiple meteors enter our atmosphere in flights, or owe their separation into discrete bodies to the effects of explosion. He remarks that several considerations seem to suggest the former theory, and quotes in its favor some telescopic observations recently made on meteoric bodies. Such observations are so seldom effected (simply because a telescope cannot be turned upon shooting stars, and the chances are enormously against the accidental passage of any of these bodies across the telescopic field of view), that great interest attaches to the few that have been recorded, especially when meteors have been seen with telescopes of considerable power. Two observations, both by Dr. Reimann, were recently announced, and the Königsberg heliometer was the instrument with which the observations were made. In the first case, three small meteors, separated from each other by small dark spaces, were seen to travel together across the telescopic field. The two in front were smaller than the third, and the three presented the appearance of a small isosceles triangle, whose base traveled in front—thus, ⁘ These bodies moved so slowly that they could be conveniently watched. This slow motion implies great distance, yet they were as bright as stars of the fourth magnitude. The observer formed no estimate of their apparent dimensions. The bodies showed no trains. In the second case, a small meteor passed across the field of view, in whose track, at a distance of about a quarter of a degree, followed a fainter meteor.

Dr. Galle remarks that the number of such observations is not large. Most of those made before the year 1860 are collected in a communication from Haidinger, read before the meeting of the Vienna Academy in February, 1861, and relating to the double meteor of Elmira and Long Island. Galle considers that if telescopic observations could be oftener effected, the number of cases of multiple meteors could be largely increased. One of the most striking instances of a multiple meteor was the one observed by Schmidt at Athens, October 18, 1863. In that case, the naked eye could recognize only what appeared to be a single meteor, but in the telescope two large meteors could be seen traveling in front of a number of small fireballs, each of which was followed by a

train. The well-known skill and accuracy of Schmidt and the length of time (14 seconds) during which the object continued in the telescopic field, renders this observation peculiarly valuable.

Dr. Galle considers that his researches into the phenomena presented by the meteors which fell at Pultusk on January 30, 1868, as a rain of stones, demonstrate that the meteors were separate long before they reached the place of so-called explosion, and that this place is only the spot where a complete resistance to the planetary velocity and a partial rebound from the impressed air take place, and whence the meteor falls with a velocity corresponding to the law of terrestrial gravity. Haidinger, from certain physical features of fallen meteors, had already inferred the necessity of the theory that the separate meteors had followed distinct paths through the air. Dr. Galle considers that at present it may be regarded as still an open question, whether meteorites enter our atmosphere, from outer space, already separated so as to form a swarm, or whether, shortly after entering and during their passage through the air, they are reduced through the effects of heat into smaller fragments, which the more or less freshly broken appearance of many fragments, as distinguished from the full or partial over-crusting of others, seems to indicate.

He notes as unusual, in the first observation by Dr. Reimann, the circumstance that the two meteors traveling in front were smaller than the one which followed them.—*Monthly Notices, Feb.*

2. *Origin of Meteoroids and Aërolites.*—An interesting speculation by Professor Schiaparelli on the hyperbolic velocities of some recently observed aërolites and fireballs,* occurs in his last published work on the "Astronomical Theory of Meteors," relating to the question of the possible identity, or of the separate origin of these meteors, and of ordinary shooting-stars or meteor-showers. Rejecting, on apparently sufficient grounds, as fallacious, the conclusion of Laplace, that if comets, before entering the sphere of the sun's attraction, are supposed to be traversing space with various velocities in various directions, the probability of their attaining the immediate neighborhood of the sun in parabolic orbits is many thousand times greater than the probability of comets with hyperbolic orbits approaching it so closely as to become visible from the earth; and adopting the exactly opposite conclusion that the frequent occurrence of parabolas, and the entire absence of hyperbolas of any very great excentricity among the orbits of non-recurrent comets indicates them all to be originally journeying in space with nearly the same velocity, and in nearly tho same direction as the sun, Professor Schiaparelli regards these bodies as the original inmates, or portions of one of the "star-drifts," of whose existence very decided proofs have lately

* Six instances of aërolitic fireballs, of which hyperbolic velocities were credibly computed, are cited by Schiaparelli in his work (translated from the Italian by Dr. von Boguslawski), *Entwurf einer Astronomischen Theorie der Sternschnuppen*, p. 207, *et seq.*

been obtained by Mr. Proctor; and as composing, with other stars of the same vast eddy, attendant bodies accompanying in its journey through space the general "drift" or star-family, of which the sun itself forms a part. On this assumption, aërolites and meteors moving with hyperbolic velocities are bodies from more distant spaces than the star-family of the sun, or wanderers from the regions of more distant star-drifts, whence they have, possibly, been projected, with sufficient initial velocities to escape from their spheres of attraction by the stars themselves; and their origin is, in this case, entirely different from that of comets, and of meteoric showers. If, as Professor Schiaparelli observes, this be the real explanation of the high velocities occasionally met with among the best recorded descriptions of aërolites and fire-balls, the evidence already obtained by the spectroscope of a general unity of composition among the remotest fixed stars is even more remarkably extended by the analysis of meteorites to the utmost limits of the starry sphere. But if the innumerable crowd and weight of the stellar fragments which this hypothesis supposes should appear to offer an insurmountable objection to its reception, the only obvious alternative remaining open to conjecture is to regard the occasionally observed high velocities of aërolites, or fire-balls, as constituting very rare exceptions; and the generality of both aërolites and shooting-stars to be moving in orbits, like the comets, with velocities which seldom greatly surpass the speed communicated to them by the sun's attraction, and as falling toward it from spaces not more distant than those of the parent eddy, or "star-stream," whose drift, or motion of translation in space, is found to be in general nearly similar to the proper motion of the sun.—*Ibid.*

IV. Miscellaneous Scientific Intelligence.

1. *The National Academy of Sciences* held its annual session at the Smithsonian Institution in Washington, D. C., April 15th–18th, 1873, when the following papers were presented:

The determination of singular points of Curves and Surfaces by the method of quarternions; Benjamin Peirce.

The geodesy of the Coast Survey; B. Peirce.

Silt analysis of Soils and Clays; E. W. Hilgard.

On the meteoric iron found in 1871, near Shingle Springs, Eldorado County, California; B. Silliman.

Experimental and graphic results of distilling certain hydrocarbons by heat, with and without the aid of vacuum and steam; C. F. Chandler and B. Silliman.

On the structure and age of the Cincinnati anticlinal; J. S. Newberry.

On the need of more accurate investigations and tables of the celestial motions; Simon Newcomb.

On the general Atmospheric circulation; A. J. Wocikof.

Determination of longitude between Europe and America, by the U. S. Coast Survey; J. E. Hilgard.

On some peculiarities in two recently discovered minor Planets; J. C. Watson.

On the altitudes of Gray's and Torrey's peaks in Colorado Territory, and some questions connected with the determination of barometric altitudes in the interior of continents; A. Guyot.

On the unity of the system of life in animals and the true principle of gradation in the various animal types; A. Guyot.

On repeating curves; H. A. Newton.

On the stability of the Meridian Circle of the Observatory of Harvard College; Joseph Winlock.

On some experiments made with a slitless Spectroscope in 1871, in order to see the whole chromosphere of the sun at once; J. Winlock.

On observations of the Sun made at the Observatory of Harvard College in 1872, with the aid of the Bache Fund; J. Winlock.

On a method of illuminating the threads of the reticule of a telescope by the electric spark; J. Winlock.

Comparison of the Spectra of the limb and of the centre of the Sun, made at the Sheffield Scientific School; Chas. S. Hastings, read by H. A. Newton.

On certain harmonies of the Solar System; Stephen Alexander.

Report of progress of a Magnetic Survey made by the aid of the Bache Fund; J. E. Hilgard.

Eulogies were also read on deceased members of the Academy: on Dr. John Torrey, by Prof. Asa Gray; on Prof. William Chauvenet, by Prof. J. H. C. Coffin.

The following members of this Academy have died during the year 1872–73: J. H. Coffin of Easton, Pa.; James Hadley of New Haven; John T. Frazer of Philadelphia; William Stimpson of Chicago; and John Torrey of New York.

The following new members were elected: Theodore Gill of Washington, D. C.; Elias Loomis of New Haven; Joseph Lovering of Cambridge, Mass.; William A. Norton of New Haven, Conn.; and J. J. Woodward of Washington, D. C.

2. *Reports of Explorations and Surveys to ascertain the practicability of a ship canal between the Atlantic and the Pacific oceans by the way of the Isthmus of Tehuantepec;* by R. W. SHUFELDT, Capt. U. S. N. 151 pp. 4to, with 20 maps. (Made under the direction of the Secretary of the Navy.) Washington, 1872.—The Report of Capt Shufeldt contains a particular account of the region surveyed, with some lithographic views. It is followed by tables of heights and distances; of Building Materials on the routes, giving the occurring rocks; of Useful Trees, Plants, &c.; of Languages of the Aboriginal Tribes, and a Report of the Hydrographic Surveys on the Atlantic Coast, by Lieut.-Comm. N. H. Farquhar, U. S. N, and on the Pacific Coast by Lieut.-Comm. A. Hopkins, U. S. N. Map No. 18 is a colored geological chart of the isthmus of Tehuantepec, showing the limits of the different rocks, but without indicating the period to which they belong.

3. *Manual of Chemical Analysis, as applied to the Examination of Medicinal Chemicals:* a guide for the determination of their identity and quality, and for the detection of impurities and adulterations. For the use of pharmaceutists, physicians, druggists, and manufacturing chemists, and of pharmaceutical and medical students. By FREDERICK HOFFMAN, Ph.D. 393 pp. 8vo, with 96 wood-cuts. 1873. New York (D. Appleton & Co.)—This title fully explains the object of Dr. Hoffman's Manual, which is a carefully prepared book, and well up to the existing state of both the science and art of modern Pharmacy. It is a book which will find its place in every medical and pharmaceutical laboratory, and is a safe and instructive guide to medical students and practitioners of medicine.

APPENDIX.

ART. LIII.—*Notice of New Tertiary Mammals* (continued); by O. C. MARSH.

THE present paper is a continuation of that on page 407. The remains here described were nearly all collected by the late expeditions from Yale College, and the type specimens are preserved in the Museum of that institution.

Tillotherium hyracoides, gen. et sp. nov.

This genus presents some remarkable characters in its dentition, which separate it widely from any described, with the possible exception of *Anchippodus* Leidy, to which it may prove to be nearly related, when additional remains of that genus are discovered. There were two large incisors in each premaxillary, the inner and larger one being gliriform, and covered on its front and outer faces with enamel. The canine was small and directed well forward. There appear to have been four upper premolars, increasing in size posteriorly. There are three true molars, the last being the largest. They are all much greater in transverse than in antero-posterior extent, and this is especially true of the last. They are composed essentially of a pair of external cones, connected with a single internal lobe by two oblique converging ridges. There is a small tubercle in the depression thus enclosed. The basal ridge on the posterior side is expanded, forming a low shelf. The antero-external cone has an outer cusp, which projects outward and forward. The present species was about two-thirds the size of a Tapir. The large upper incisors are sub-triangular in transverse outline, the posterior face being concave. The lower jaws and skeleton are not known with certainty. It is possible that the present remains may prove to be generically identical with *Anchippodus minor* Marsh (*Trogosus castoridens* Leidy).

Measurements.

Antero-posterior diameter of large upper incisor,	21·	mm.
Transverse diameter,	15·	
Space occupied by last three upper molars,	59·	
Antero-posterior diameter of first upper true molar,	16·5	
Transverse diameter,	29·	
Antero-posterior diameter of last upper molar,	21·	
Transverse diameter,	38·	

The known remains of this species are all from the Eocene of Wyoming.

Brontotherium gigas, gen. et sp. nov.

An examination of the remains, in the Yale Museum, of the huge mammals allied to *Titanotherium*, has led to the discovery that two different animals have hitherto been referred to the species known as *T. Prouti*. These animals are generically distinct, and probably are from separate geological horizons. The one here described differs from *Titanotherium* in its dentition, having but three lower premolars, the series being as follows:—Incisors 2, canine 1, premolars 3, molars 3. The animal was, moreover, a true Perissodactyl, with limb-bones resembling those of *Rhinoceros*. The genus is related to *Titanotherium* * and the two appear to form a distinct family, which may be called *Brontotheridæ*.

The present species is based on portions of three individuals, one of which has the lower jaws and entire molar series complete. They indicate an animal fully equal to *T. Prouti* in size, and but little inferior in bulk to the Elephant. The lower molars resemble those in the type specimen of *T. Prouti*, but the jaw below them is not so deep, and its lower margin is more nearly straight, descending but very slightly toward the angle. The front part of the lower jaws is somewhat suilline in form. The incisors are quite small, and the two next to the symphysis are separated from each other. There is a short diastema between the canine and first premolar.

From the other specimens preserved, the greater part of the skeleton can be made out. It closely resembles that in recent Perissodactyls, but shows some approach to the Proboscidea. The femur has a third trochanter, and its head a pit for the round ligament. The fibula is entire, and slender. The astragalus is remarkably short. It has a deep groove on its upper surface, and the articular facets for the navicular and cuboid are nearly equal. In the manus there are four toes of nearly equal size, the first digit being rudimentary or wanting. There were three digits only in the pes, the first and fifth being entirely wanting. The toes were short and thick, as in Proboscidians. The metacarpals and metatarsals are longer than in the elephant, and the phalanges shorter. The foot was also more inclined. The carpal and tarsal bones are very short, and form interlocking series. The tail was long and slender.

* The generic name *Titanotherium* Leidy is antedated by *Menodus* Pomel (Bib. Univ. de Genève, x, p. 75, Jan., 1849). The latter, however, is essentially the same word as *Menodon* von Meyer, 1838, and is also objectionable in its form; hence *Titanotherium* should be retained.

Measurements.

Length of lower jaw, from condyle to front of symphysis,	634·	mm.
Depth of lower jaw, from top of coronoid process to angle,	367·	
Depth below front of last lower molar,	123·	
Depth below first lower molar,	113·	
Length of symphysis,	122·	
Length of last lower molar,	117·	
Length of last lower premolar,	51·	
Transverse diameter,	35·	
Length of first lower premolar,	31·	
Transverse diameter,	21·	
Distance between lower canines,	28·5	

The remains on which the above description is based were found in the Miocene of Colorado by Mr. H. B. Sargent, Mr. J. W. Griswold, and the writer.

Elotherium crassum, sp. nov.

A large suilline mammal, which probably belongs in the genus *Elotherium*, is indicated by portions of two skeletons, in the Yale Museum. These specimens present some features not before observed in any Ungulates. The most striking of these is a very long process descending from the malar bone, and giving attachment to the masseter muscle. This process resembles somewhat the downward prolongation from the zygomatic arch in some Edentates and Marsupials, but it is longer, and more compressed. The radius and ulna were separate, or very loosely united. The third and fourth metacarpals were nearly equal in size, and the second and fifth larger than the corresponding bones of the pes. In the latter the first digit was wanting, and the fifth rudimentary. The hoof phalanges were short. The tail was long, and quite slender. This species is intermediate in size between *E. Mortoni* and *E. ingens*.

Measurements.

Length of malar process below squamosal suture,	130·	mm.
Length of symphysis of lower jaws,	144·	
Antero-posterior diameter of lower canine,	32·5	
Transverse diameter,	28·5	
Transverse diameter of humerus at distal end,	81·	
Transverse diameter of radius at distal end,	75·	
Transverse diameter of head of tibia,	81·	
Length of third metatarsal,	102·	

A rather smaller specimen, apparently of the same species, afforded the following

Measurements.

Length of symphysis,	122·	mm
Depth of jaw below first premolar,	57·	
Depth below last lower molar,	69·	
Space occupied by lower molar series,	216·	
Space occupied by three lower true molars,	76·	

All the known remains of the present species are from the Miocene of Colorado.

Yale College, May 5th, 1873.

INDEX* TO VOLUME V.

A

Academy National, meeting, Nov., 1872, 78.
annual session, 483.
Agardh, J. G., botanical work, noticed, 144.
Agassiz, A., Revision of the Echini, noticed, 185.
History of Balanoglossus and Tornaria, noticed, 234.
Aitken, J., glacial motion, 305.
Alizarine, natural and artificial, 160.
Alizarin, method of preparing, 299.
Allman, G. J., Monograph of the Gymnoblastic Hydroids, noticed, 145.
Amides and nitrides, modes of forming, 132.
Anilin, conversion into toluidin, 134.
Anthracene, synthesis of, 298.
Astronomical Engravings from the observatory of Harvard, noticed, 319.
Astronomische Nachrichten, 321.
Auroral spectrum, 320.
Aurora, spectrum of, *Barker*, 81.
Auroras, solar spots and magnetic declination, *Loomis*, 245.
Azodiamines, coloring matters of aromatic, 379.

B

Baillon, H., origin and characters of officinal rhubarb, 141.
Balance, new platform, 136.
Barker, G. F., spectrum of the aurora of Oct. 14, 1872, 81.
chemical abstracts, 63, 296, 377.
Battery, determining resistance of, *Hodges*, 375.
Benzol, inferior homologue of, 65.
Biedermann and *Oppenheim*, bromide of terpene, 132.
Bigelow, F. H., method of measuring induced currents, 374.
Boiler incrustation, *Koenig*, 299.
Botanical necrology, 1872–3, 391.
Botanical abstracts, *Gray*, 75, 142, 316, 389, 479.
Botany—
Baillon's Histoire des Plantes, 145.
Bentham and Hooker, Genera Plantarum, 480.
Boissier's Flora Orientalis, 142.
Caulerpa group, new genus of, 144.
Charæ, calcareous-encrusted, 75.
Botany—
Chlorodictyon, 144.
Coniferæ, theoretical structure of cone in, 75.
Gelsemium has dimorphous flowers, *Gray*, 480.
Gramineæ, Tieghem on cotyledon of, 389.
Grasses, fertilization of, 316.
Hamamelis, discharge of seeds of, 144.
Hooker's Icones Plantarum, 143, 480.
Journal of, British and Foreign, 143.
Linnæan Soc., Journal of, 143.
Lolium, infelix, 390.
Marsilia and Pilularia, Braun on, 145.
Mélastomacées, Triana on, 145.
Nervation of coats of ovules and seeds, 479.
Pollen, small bodies in the fovilla of, 390.
Rhubarb, officinal, origin and characters of, *Baillon*, 141.
Rubus Idæus, American origin of, 479.
Saccardo on certain small bodies in the fovilla of pollen, 390.
Sachs' Lehrbuch, noticed, 397.
Textile plant, new, 480.
Willow, weeping, origin of, 75.
Witch-hazel, discharge of seeds of, 144.
Zizania aquatica not tuberiferous, 75.
Brush, G. J., compact anglesite from Arizona, 421.

C

Cannizzaro, theoretic teaching of chemistry, 296.
Carpenter, W. B., Dardanelles and Bosphorus under-current, 240.
Chalanger expedition, 401.
Champion and *Pellet*, relation between vibration and detonation, 297.
Chapman, H. C., Evolution of Life, noticed, 322.
Chase, A. W., Oregon borate of lime (Cryptomorphite?), 287.
Chemistry, theoretic teaching of, 296.
Chromium, quantitative estimation of and separation from uranium, *Gibbs*, 110.
Chromosphere, new method of viewing, 319.
visible in small telescopes, 319.
Clarke, W. B., address, noticed, 323.
Clerc, F. Le, and D. de Bénazé, on rate of growth of coral reef, noticed, 74.

* The Index contains the general heads Botany, Geology, Mineralogy, Zoology, and under each the titles of Articles referring thereto are collected.

Clève and *Hoeglund*, combinations of yttrium and erbium, 133.
Coan, T., eruption on Hawaii, 476.
Cœrulignone, 298.
Cooke, G. H., geological report, noticed, 478.
Cope, E. D., Cretaceous of Wyoming, 230.
dates of some recent papers of, *Marsh*, 235.
Coral, rate of growth of, at Tahiti, *Bénazé*, 74.
Coues, E., Key to N. American Birds, noticed, 314.
Cox, E. T., meteorite in Indiana, 155.
geological report, noticed, 233.
Credner, H., Elemente der Geologie, noticed, 73.
Crookes, W., Wagner's Chemical Technology, noticed, 159.
Currents, induced, and derived circuits, *Trowbridge*, 372.
methods of measuring, *Bigelow*, 374.
Cymol from oil of turpentine, and oil of lemons, 132.

D

Dana, J. D., quartzite, limestone, and associated rocks of Great Barrington, Mass., 47, 84.
Glacial and Champlain eras in New England, 198, 217.
Cainozoic versus Cænozoic or Cenozoic, 233.
origin of mountains, notice of Hall's theory of, 347.
results of earth's contraction, origin of mountains, 423, 474.
Dardanelles and Bosphorus under-current, *Carpenter*, 240.
Darwin, C., Voyage of the Beagle, noticed, 234.
Expression of the Emotions, noticed, 397.
Dawkins, W. B., classification of Pleistocene strata by the mammalia, 303.
Dawson, J. W., impressions, footprints, etc., on Carboniferous rocks, 16.
Debray, purple of Cassius, 378.
Delesse, A., Lithologie des Mers, 73.
Denza, meteoric shower of Nov. 27–28, 1872, in Italy, 126.
Detonation, and vibration, relation between, 297.
Dextrine, 64.
Diffraction-gratings, photographs of, 216.
Dorp, von, synthesis of anthracene, 298.
Draper, J. W., distribution of chemical force in the spectrum, 25, 91.

E

Earth's contraction from cooling, results of, 219, 423, 474.
Earthquakes, recent, *Rockwood*, 260.
Eaton, J. H., relations of the sandstones, conglomerates and limestones of Sauk Co., Wisc., 444.
Earthquake waves, *Hilgard*, 308.
Edinburgh Geological Soc., Transactions, noticed, 478.
Estes, E., Half-hour recreations in Popular Science, noticed, 406.
Evans, J., Ancient Stone Implements, etc., of Great Britain, noticed, 322.
Explorations west of the 100th meridian, *Yarrow*, 290.
Eye, sensibility of, to intensity of different colors, 380.

F

Fatty acids, hydrates of monobasic, 299.
Ferrel, W., meteorological effects upon the heights of tides, 342.
Feuchtwanger, L., treatise on gems, noticed, 80.
Filtering apparatus, automatic, *Wiley*, 350.
Filter-pump, improved, *Thorpe*, 216.
Fittig, new coal-tar hydrocarbons, 136.
Ford, S. W., fossils from the Primordial of Rensselaer Co., N. Y., 211.
Fossil, see GEOLOGY.
Fox, C. B., Ozone and Antozone, 381.

G

Gabb, W. M., notes on the Island of Curaçao, 382.
Galle, telescopic observations of meteors, 481.
Galvanometer, device for projecting deflections of, *Mayer*, 270.
Gannett, H., List of Elevations, noticed, 405.
Gas wells, *Newberry*, 225.
Geinitz, H. B., Elbthalgebirge in Sachsen, noticed, 160, 478.
Geological Report of Canada, noticed, 477.
Indiana, 233.
Minnesota, 313.
New Jersey, 478.
Ohio, 478.
Geological Survey of the Territories for 1873, 475.
GEOLOGY—
Birds, new sub-class of, *Marsh*, 161.
Cainozoic versus Cænozoic or Cenozoic, 233.
Carboniferous, footprints, etc., *Dawson*, 16.
Carboniferous of Pennsylvania, footprints in, *Moore*, 292.
Champlain and Glacial eras in New England, *Dana*, 198, 217.
Colorado, country north of Grand Cañon of, *Powell*, 456.

GEOLOGY—
Contraction, result of the earth's, *Mallet*, 219, *Dana*, 423, 474.
Cretaceous, eastern limit in Iowa, *White*, 66.
N. America, birds from, 229.
Utah, Meek on, 310.
Wyoming, *Cope*, 230.
Curaçao I., notes on, *Gabb*, 382.
Dinocerata, additional observations on, *Marsh*, 293.
note on, *Marsh*, 310.
Dynamical, history of certain recent views in, *Mallet*, 302.
some points in, *Hunt*, 264.
Gas wells, *Newberry*, 225.
Icthyornis, new species, *Marsh*, 74.
Mammals, gigantic fossil, of the order Dinocerata, *Marsh*, 117.
Man, supposed evidences of, in the Miocene, 479.
Mastodon in Ohio, *Hicks*, 79.
Metamorphic rocks, age of, in Wisconsin, *Irving*, 282.
Metamorphism, *Wurtz*, 385.
Miocene, Vertebrata from, *Leidy*, 311.
Mountains, origin of, *Dana*, 423, 474.
Pleistocene strata, classification of, *Dawkins*, 303.
Primordial fossils from, *Ford*, 211.
Quartzite, limestone, etc., of Great Barrington, Mass., *Dana*, 47, 84.
Salt deposits of Western Ontario, *Gibson*, 362.
Sauk Co., Wis., relations of the sandstones, conglomerates and limestones of, *Eaton*, 444.
Spergen Hill fossils from Idaho, *Meek*, 383.
Tertiary mammals, new, *Marsh*, 407, 485.
Trias in British Columbia, *Whitney*, 473.
Wahsatch Mts., Devonian fossils in, *Tenney*, 139.
Wyoming, age of certain beds of, *Lesquereux*, 308.
Zaphrentis, spontaneous fission? in, *White*, 72.
Gibbs, W., quantitative estimation of chromium and separation from uranium, 110.
estimation of magnesium as pyrophosphate, 114.
chemical abstracts, 131.
Gibson, J., salt deposits of Western Ontario, 362.
Gill, T., arrangement of the families of Fishes and Mammals, noticed, 315.
Girard, method for preparing alizarin, 299.
Glacial and Champlain eras in New England, *Dana*, 198, 217.
Glacial motion, *Aitken*, 305.
Glaciers, ancient, of the Sierras, 325.
in California, 69, 325.
trains of boulders, etc., *Reed*, 218.
Glover, T., work on Orthoptera, noticed, 148.
Goldsmith, E., trautwinite, 313.
Gray, A., botanical abstracts, 75, 142, 316, 389, 479.
biographical notice of John Torrey, 411.
Galsemium has dimorphous flowers, 480.
Gregory, T. F., tin in Queensland, 137.
Grimaux, hydrates of monobasic acids, 299.

H

Hall, N. P., pitchblende and tellurium-gold ore in Colorado, 386.
Harger, O., sexes of Sphæroma, 314.
Hastings, C. S., comparison of the spectra of the limb and center of the sun, 369.
Haughton, S., differences between a hand and a foot as shown by their flexor tendons, 148.
Heat, dynamical theories of, *Norton*, 186.
Helmholtz, sensibility of the eye to intensity of different colors, 380.
Hessenberg, F., mineralogical work, 314.
Hicks, L. E., Mastodon in Ohio, 79.
Hildebrand, fertilization of grasses, 316.
Hilgard, J. E., note on earthquake waves, 308.
Hill, G. W., papers on transit of Venus, noticed, 319.
Hodges, N. D. C., methods of determining resistance of a battery, 375.
Hoffman, F., Manual of chemical analysis, noticed, 484.
Hofmann, A. W., conversion of anilin into toluidin, 134.
coloring matters from aromatic azodiamines, 379.
Holley, G. W., work on Niagara, 79.
Hough, F. B., meteorology of New York, work on, noticed, 240.
Hull, E., treatise on building and ornamental stones, noticed, 234.
Huggins, spectrum of the nebula of Orion, 75.
Hunt, T. S., some points in dynamical geology, 264.
reply to criticisms of, *LeConte*, 448.
Hydrocarbons, new coal-tar, 136.
Hydrogen, specific heat of occluded, 377.

I

Iron, galvanic reduction of, 380.
Irving, R. D., age of metamorphic rocks in Wisconsin, 282.

J

Jacobi, galvanic reduction of iron, 380.
Johnson, S. W., chemical notices, 136.
Jones, F., chemical text book noticed, 322.
Jungfleisch, transformation of right tartaric acid into racemic, 134.
Jupiter and its satellites, *Mitchell*, 454.

K

Kekulé and *Franchimont*, triphenylmethane, 135.
Kobell, F. v., tables for determining minerals, noticed, 478.
Koenig, G. A., boiler incrustation from New Jersey, 299.
Kokscharow, mineralogical work noticed, 140.

L

Landenburg, triethylmethane, 135.
Landolt, method of determining molecular weights from the vapor volume, 65.
Le Conte, J., note, 156.
ancient glaciers of the Sierras, 325.
formation of the earth-surface, reply to Hunt, 448.
Leets, new modes of forming amides and nitrides, 132.
Leidy, J., fossil Vertebrata from Miocene of Virginia, 311.
Lesquereux, L., age of certain beds in Wyoming referred to the Tertiary or Cretaceous, 308.
Liebermann, cœrulignone, 298.
Lightning, duration and multiple character of flashes of, *Rood*, 163.
Liquids, height to which they may be heaped above the edge of a vessel, *Mendenhall*, 129.
Lockyer, J. N., spectrum of sun, noticed, 236.
Logarithms, erratum in Sang's, 406.
Loomis, E., instances of low temperature at New Haven, 238.
comparison of range of magnetic declination and number of auroras with extent of spots on sun, 245.

M

Magnesium, estimation of as pyrophosphate, *Gibbs*, 114.
Magnetization, effects of, in changing the dimensions of iron, steel, and bismuth bars, etc., *Mayer*, 170.
Mallet, R., origin of heat of volcanoes, etc., 140, 219.
history of certain recent views in dynamical geology, 302.
Marsh, O. C., new species of Icthyornis, 74.
gigantic fossil mammals of the order Dinocerata, 117.
Marsh, O. C., new sub-class of fossil birds (Odontornithes), 161.
birds from Cretaceous of N. America, 229.
dates of some of Prof. Cope's recent papers, 235.
additional observations on Dinocerata, 293.
supplementary note on Dinocerata, 310.
new Tertiary mammals, 407, 485.
return of geological expedition of, 71.
Martin, J. H., Manual of microscopic mounting, noticed, 80.
Mayer, A. M., determination of the relative intensities of sounds, etc., 44, 123.
effects of magnetization in changing the dimensions of iron, steel and bismuth bars, etc., 170.
device for projecting deflections of galvanometer, 270.
Earth a great Magnet, noticed, 157.
Meek, F. B., Cretaceous in Utah, 310.
Spergen Hill fossils from Idaho, 383.
Mendenhall, T. C., determination of the height to which liquids may be heaped above the edge of a vessel, 129.
Meteoric iron, Victoria, *Smith*, 107.
Meteorite in Indiana, *Cox*, 155.
Meteor in Kentucky, 318.
double, of Feb. 14, 1873, 318.
Meteoroids and aërolites, origin of, 482.
Meteors of Nov. 24–27, 1872, *Newton*, 53.
and Biela's comet, *Newton*, 317.
as observed in Italy, Denza, 126.
in Europe, 150.
telescopic observations of, 481.
MINERALS—
Anglesite, compact, from Arizona, *Brush*, 421.
Borate of lime, Oregon, *Chase*, 287.
Diamond, large, 313.
Diamonds, in hydraulic washings in California, *Silliman*, 384.
Chladnite, 107.
Cryptomorphite?, *Chase*, 287.
Enstatite, 107.
Pitchblende in Colorado, 386.
Tellurium ores in Colorado, 386.
Tin in Queensland, *Gregory*, 137.
Trautwinite, 313.
Mitchell, M., observations on Jupiter and its satellites, 454.
Moore, W. D., footprints in Carboniferous of Pennsylvania, 292.
Mountains, origin of, *Dana*, 347, 423.
Muir, J., Glaciers in California, 69.
Musculus, dextrine, 64.

N

Naumann on pseudomorphism, 312.
Newberry, J. S., gas wells, 225.

Newberry, J. S., geological report noticed, 477.
Newton, H. A., meteors of Nov. 24th–27th, 1872, 53, 150.
meteors of Nov. 27th, 1872, and Biela's comet, 317.
double meteor of Feb. 14, 318.
New York Central Park, notice of report, 404.
Nicholson, H. A., Manual of Paleontology, noticed, 233.
Contributions to a Fauna Canadensis, noticed, 386.
Nitriles, and amides, mode of forming, 132.
Nitrogen, action of charcoal on organic, 377.
spectrum of, 131.
Norton, W. A., dynamical theories of heat, 186.

O

Obituary—
Brebisson, L. A. de, 395.
Coffin, J. H., 242.
Curtis, M. A., 391.
Gris, Arthur, 396.
Liebig, J., 406.
Maury, M. F., 242.
Mohl, Hugo von, 393.
Œrsted, A. S., 396.
Rankine, Macquorn, 242.
Reuter, G. F., 395.
Sedgwick, Adam, 242.
Somerville, Mary, 241.
Spring, F. A., 393.
Sullivant, Wm. S., 481.
Torrey, John, 324, 411.
Welwitsch, Frederick, 396.
Observatory, U. S. Naval, reports, 320
Oppenheim, cymol from oil of terpentine and oil of lemons, 132.
Orcin, synthesis of, 65.
O'Sullivan, transformation-products of starch, 63.
Oxygen, free, determination of, in solution, 379.

P

Palmieri and Mallet, eruption of Vesuvius in 1872, noticed, 140.
Paraffin, action of heat and pressure on, 66.
Parasulphobenzoic acid, *Remsen*, 179, 274, 354.
Peters, C. H. F., new planet, 215, 243.
Watson, 62.
Powell, J. W., geological structure of country north of Grand Cañon of Colorado, 456.
Purple of Cassius, 378.
Pyroligneous acid, coloring matter from, 298.

R

Remsen, I., investigations on parasulphobenzoic acid, 179, 274, 354.
Ramsay, A. C., Physical Geology and Geography of Great Britain, 72.
Reed, S., trains of boulders, and transport of boulders to level above their source, 218.
Ridgway, R., relation between color and geographical distribution in birds, 39.
Roberts and *Wright*, specific heat of occluded hydrogen, 377.
Rockwood, C. G., recent earthquakes, 260.
Rommier, inferior homologue of benzol, 65.
Rood, O. N., duration and multiple character of flashes of lightning, 163.
Rowland, H. A., auroral spectrum, 320.

S

Sachs, J., Lehrbuch, noticed, 397.
Schoot, C., tables of rain and snow in the U. S., noticed, 239.
Schuster, spectrum of nitrogen, 131.
Schützenberger and *Gérardin*, determination of free oxygen in solution, 379.
Scientific series, international, noticed, 241.
Secchi, P. A., spectroscopic notes, noticed, 321.
variation in diameter of sun, 397.
Selenium, effect of light on, during passage of electric current, 301.
Selwyn, A. R., geological report noticed, 477.
Shufeldt, R. W., Explorations of the Isthmus Tehuantepec, noticed, 484.
Silliman, B., microscopic diamonds, with zircons and topaz, in hydraulic washings in California, 384.
Smith, J. L., Victoria meteoric iron, and on chladnite or enstatite, 107.
Solar, see *Sun*.
Sounds, relative intensities of, etc., *Mayer*, 44, 123.
Spectroscope, photographs of diffraction-gratings, 216.
use of diffraction-grating for solar, *Young*, 472.
Spectrum of aurora, *Barker*, 81.
Rowland, 320.
distribution of chemical force in, *Draper*, 25, 91.
of the nebula of Orion, 75.
nitrogen, 131.
sun, Lockyer's work noticed, 236.
Spencer, H., Philosophy and Morals, work on, noticed, 406.
Stanford, action of charcoal on organic nitrogen, 377.
Starch, transformation-products of, 63.

Stevenson, J. J., Upper Coal measures west of the Alleghany Mts., noticed, 477.
Strutt, J. W., photographic reproduction of diffraction-gratings, 216.
Sun spots, magnetic declination and auroras compared, *Loomis*, 245.
Sun, variation in diameter of, 397.
Sweden, geological chart of, noticed, 140.

T

Tartaric acid, right, transformation into racemic acid, 134.
Tellurium-gold ore in Colorado, 386.
Temperature, low, at New Haven, *Loomis*, 238.
Tendons, flexor, difference between in hand and foot, *Haughton*, 148.
Tenney, S., Devonian fossils in the Wahsatch Mts., 139.
Terpene, bromide of, 132.
Thompson, C. W., Challenger expedition, 401.
Depths of the Sea, noticed, 399.
Thomson and Tait, Elements of National Philosophy, noticed, 381.
Thorpe and *Young*, action of heat and pressure on paraffin, 66.
Thorpe, improved filter-pump, 216.
Tides, meteorological effects upon the heights of, *Ferrel*, 342.
Tieghem on cotyledon of Gramineæ, 389.
Torrey, John, biographical notice, *Gray*, 411.
Triethylmethane, 135.
Triphenylmethane, 135.
Trowbridge, J., induced currents and derived circuits, 372.
Triana, J., les Mélastomacées, noticed, 145.
Tyndall Endowment, 398.
Tyndall, works, noticed, 323.

U

Ultramarine, constitution of, 135.
Unger, constitution of ultramarine, 135.

V

Vancouver and Queen Charlotte Isls., explorations in, 231.
Vapor volume, determining molecular weights from, 65.
Vibration and detonation, relation between, 297.
Volcanoes of Hawaii, eruption, 72; *Coan*, 476.
Verrill, A. E., results of recent dredging on the coast of N. England, 1, 98.
certain errors in Mr. Jeffreys' article on the mollusca of Europe and N. America, 465.
Versmann, F., alzarine, work on, 160.
Vesuvius, eruption of, work of Palmieri on, noticed, 140.
Vogt and *Henninger*, synthesis of orcin, 65.

W

Wanklyn, tests for certain organic fluids, 63.
Watson, J. C., discovery of new planet, 62.
Wheeler, G. M., explorations, noticed, 237, 290.
White, C. A., eastern limit of the Cretaceous in Iowa, 66.
spontaneous fission? in Zaphrentis, 72.
Whitney, J. D., occurrence of the Trias in British Columbia, 473.
Wiley, H. W., automatic filtering apparatus, 350.
Winchell, N. H., geological report, noticed, 313.
Wisconsin Academy, Transactions, noticed, 160.
Woodward, H., monograph of British fossil crustacea, noticed, 312.
Wood, H. C., work on fresh water algæ, noticed, 391.
Wurtz, H., metamorphism as a consequence of transformation of motion into heat, 385.

Y

Yarrow, H. C., explorations west of the 100th meridian, 290.
Yellowstone National Park, photographs, noticed, 79.
Young, C. A., diffraction-grating as a substitute for prisms in a solar spectroscope, 472.
Yttrium and erbium, combinations of, 133.

Z

Zoology—
Balanoglossus and Tornaria, history of, noticed, 234.
Birds, new sub-class of, *Marsh*, 160.
Birds, relation betwen color and geographical distribution, *Ridgway*, 39.
Coral reef, rate of growth of, 74.
Dredging, results of, on the coast of N. England, *Verrill*, 1, 98.
Echini, revision of, noticed, 158.
Fishes, Gill's classification of, 315.
Hydroids, Allman's work, noticed, 145.
Invertebrates, marine, from N. England, 1, 98.
Mammals, Gill's classification of, 316.
Mollusca of Europe compared with those of N. America, *Verrill*, 465.
Sphæroma, sexes of, *Harger*, 314.
See further, under Geology.